we must take issue as energetically against the theory of intuition as against his pragmatism (page 54).

I have not formulated the above considerations systematically but have rather adopted the rhetorical style of the French in order to remain as objective as possible. It seems to me the time has not yet come for a far-reaching reflective critique, since Bergson has promised a more conclusive argument for his theory in the future. In any case he must without question come to an understanding with Kant; for to uphold metaphysics according to Kant is difficult, but to introduce intuition again is by far the most difficult.

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MAUPERTUIS AND THE PRINCIPLE OF LEAST ACTION.

The present investigations are concerned with the history of the Principle of Least Action in the hands of Maupertuis, Euler and others. The subject is of great importance in the history of mechanics, both because the principle of least action became, in the hands of Lagrance, "the mother," as Jacobi expressed it, "of our analytical mechanics," and because the animistic tendency displayed in the search for a maximum or a minimum principle in physics undoubtedly had a great influence on such moulders of mechanical theory as Euler, Lagrange (in his early work).[1] Hamilton, Gauss, and, in

[1] Besides Lagrange's early printed works, his correspondence with Euler allows us to form some impression of the stimulating effect which the principle of least action had on Lagrange's mind at the beginning of his career. Lagrange's correspondence with Euler extends from 1754 (probably: the year is not given) to 1775 and is reproduced in the *Œuvres de Lagrange*, vol. xiv, pp. 133-245. Already in 1754 Lagrange announces (*ibid.*, p. 138) that he has made "some observations about the maxima and minima which are in the actions of nature." In a letter of August 12, 1755 (*ibid.*, pp. 138-139) Lagrange informs Euler that he had a new and simpler method of solving isoperimetrical problems and (*ibid.*, pp. 140-144) gives a full statement of it (cf. Euler's reply, *ibid.*, pp. 144-146). This discovery of what was afterwards called "the calculus of variations" certainly gave the principle of least action an additional attractiveness to Lagrange; he speaks, in a letter of May 19, 1756, of his meditations "on the application of the principle of least action to the whole of dynamics" (*ibid.*, p. 155; cf. pp. 156, 158, 161, and the final sentences of Lagrange's first printed paper in the first volume of his *Œuvres*). Lagrange's interest in the principle of least action seems to have evaporated when he observed that, when developed, the integrand is the variational form of d'Alembert's principle, and that it is simpler and equally effective to start with the equations of motion divorced from the integration. This is Lagrange's point of view in 1788. The earliest date at which this change in point of view is shown is, so far as I can find, 1764 (early memoir on the libration of the moon). In a letter of Sept. 15, 1782, to Laplace, Lagrange

our own times, Willard Gibbs. I have avoided, as much as possible, entering into merely biographical details and details of the great controversy between Maupertuis, König, Euler, and Voltaire about this very principle, in so far as they have no value in the history of science. But I have been very careful to give accurate and detailed references to the books and memoirs where everything relevant, so far as I know, may be found. I mention this expressly, because much in this chapter of the evolution of mechanics—one may even say, of thought in general—has been misquoted or misunderstood by even eminent authorities. Unless the contrary is stated, all the books referred to have been consulted either by my assistant, Miss Harwood, or by myself.[2]

I.

Pierre Louis Moreau de Maupertuis[3] was born at Saint-Malo in 1698 and died at Basel in 1759. He was the first French Newtonian;[4] was the author of several papers on the figure of the earth and the leader of that well-known French expedition which measured an arc of the meridian in Lapland, confirming the deduction from the Newtonian theory that the earth is flatter at the poles;[5]

says (*Œuvres*, vol. xiv., p. 116) that he has almost finished a mechanical treatise uniquely founded on "the principle or formula" given in section 1 of his memoir of 1780 on the libration of the moon.

[2] Adolf Mayer (*Geschichte des Princips der kleinsten Action.* Akademische Antrittsvorlesung, Leipsic, 1877, p. 7) reports that among the manuscripts left by Jacobi are fragments of a history of the principle of least action of which he has made use.

[3] There is a biography of Maupertuis by La Beaumelle (*Vie de Maupertuis par L. Angliviel de la Beaumelle; ouvrage posthume, suivi de lettres inédites de Frédéric le Grand et de Maupertuis, avec des notes et un appendice,* Paris, 1856). Cf. also Samuel Formey, *Eloge de M. de Maupertuis* (read in 1760), reprinted, with additions and corrections by de la Condamine and Trublet, in 1766 in the *Histoire de l'Académie de Berlin* for 1759, pp. 464-512; and Emil du Bois-Reymond, *Maupertuis; Rede....,* Leipsic, 1893 (on La Beaumelle's book, see pp. 72-81).

[4] La Beaumelle, *op. cit.,* p. 16; du Bois-Reymond, *op. cit.,* pp. 17-18. See Maupertuis's papers in the Paris *Mémoires* for 1732-1736; and *Discours sur les différentes figures des astres, avec une exposition des systèmes de MM. Descartes et Newton,* published anonymously at Paris in 1732 and again in 1742 (not seen), and the popular part of it is most conveniently consulted in the *Œuvres de Mr. de Maupertuis,* Lyons, 1756, vol. i, pp. 79-170. Cf. La Beaumelle, *op. cit.,* pp. 23-34; I. Todhunter, *A History of the Mathematical Theories of Attraction and the Figure of the Earth from the Time of Newton to that of Laplace,* London, 1873, vol. i, pp. 63-76, 93-102 (this also contains an account of those works which come into the scope of the next note).

[5] La Beaumelle, *op. cit.,* pp. 34-64, 71-75, 457-458, 461-462, 467; Du Bois-Reymond, *op. cit.,* pp. 18-35; and a German translation with notes by myself, of Clairaut's book of 1743 on the figure of the earth, which is soon to appear in *Ostwald's Klassiker.*

and was Frederick the Great's President of the Berlin Academy[6] (from 1746). With Maupertuis's geometrical works we are not concerned here,[7] nor are we with those philological and anatomical speculations of his, which were so ruthlessly and unjustly parodied by Voltaire.

According to Du Bois-Reymond,[8] Maupertuis's teleological tendencies showed themselves early in his career in speculations as to what grounds the Creator could have had for preferring the law of the inverse square to all other possible laws of attraction.

Some words about Maupertuis's personal character are necessary. When Maupertuis returned from Lapland, there was great opposition in some quarters to the reception of his results. This foolish opposition had a bad influence on Maupertuis: his never small feeling of self-importance increased, and he became embittered.[9] On the other hand, he was given, as President of the Berlin Academy, almost unlimited powers, even as regards the payment of the members' pensions,[10] and this may partly explain, as Carlyle suggests in his *Frederick the Great,* the tiring chorus of praise that breaks out in the Berlin *Histoire* whenever any of the members have occasion to mention Maupertuis's name. In the course of our discussions, too, we shall have, in order to correct a strange error about Maupertuis and the principle of least action made by Lord Morley in his well-known book on *Diderot and the Encyclopædists,* to touch upon the question as to whether Maupertuis was a materialist or not.[11]

II.

Maupertuis read to the Paris Academy on the 20th of February, 1740, a memoir entitled: "Loi du Repos des Corps."[12] He began by remarking that demonstrations *a priori* of such principles as that

[6] La Beaumelle, *op. cit.*, pp. 65-68, 76, 91-98, 104; du Bois-Reymond, *op. cit.*, pp. 36, 38, 39-42.

[7] La Beaumelle, *op. cit.*, pp. 15-16. 18-19, 22-23, 460-461; du Bois-Reymond, *op. cit.*, p. 16; M. Cantor, *Vorlesungen über Geschichte der Mathematik,* vol. iii, 2d ed., Leipsic, 1901, pp. 774-775, 786.

[8] *Op. cit.*, p. 18. The place where this speculation is given is in the *Figure des Astres* (*Œuvres,* 1756, vol. i, pp. 166-170).

[9] Du Bois-Reymond, *op. cit.*, p. 33.

[10] *Ibid.*, p. 40; La Beaumelle, *op. cit.*, p. 107.

[11] In the course of this article, we shall refer to Mach's work on mechanics as *Mechanik* and *Mechanics,* as we have done before (*Monist,* April, 1912).

[12] *Histoire de l'Académie royale des sciences. Année 1740. Avec les Mémoires de Math. et de Phys. pour la même Année,* Paris, 1742, pp. 170-176; *Œuvres,* 1756, vol. iv, pp. 45-63.

of the conservation of *vis viva* "cannot apparently be given by physics; they seem to belong to some higher science."

Maupertuis sought for a general law in statics analogous to the known theorem that, in any system of elastic bodies in motion, which act upon one another, $\Sigma m.v^2$ is constant, and found that: In order that a system of bodies of which each is attracted to a center by a force varying as the nth power of the distance from that center, should remain in equilibrium, it is necessary that

$$\Sigma m.f.z^{n+1},$$

where f is the intensity of the force which acts on m, and z is the distance of the mass m from its center of force, is a maximum or a minimum. In the proof, by showing the truth of the principle in two classes of cases, he concludes that as, for equilibrium

$$\Sigma m.f.z^n.dz = 0,$$

the above sum must be a maximum or a minimum.[13]

In an "Addition" added to the reprint in the *Œuvres*,[14] Maupertuis remarked that his law holds if the forces are proportional to functions Z of the distances z, and then the law is that

$$\Sigma m.f.\int Z.dz$$

must be a minimum.[15]

III.

Maupertuis's first enunciation of the law of the least quantity of action was in a memoir read to the French Academy on April 15th, 1744, entitled: "Accord de différentes Loix de la Nature qui avoient jusqu'ici paru incompatibles."[16] The laws in question ap-

[13] If there is one constant force on all the masses, and its center is at an infinite distance from the system, the center of gravity of the system must be as far as possible from, or as near as possible to, this center, for equilibrium to subsist.

[14] Vol. iv, pp. 62-63. It should be remarked that Euler, in a paper quoted below in the Berlin *Histoire* for 1751, pp. 171-173, had pointed out: (1) that it is not necessary that the forces are proportional to like powers of the distances, provided that we do not neglect the coefficients $1/(n+1)$ when they are different for the different bodies on which the forces act (p. 171); (2) that the forces need not be supposed to be proportional to functions (*fonctions quelconques*) of the distances, and if the force is V instead of fz^n, $\Sigma f.m.V.dz$ will then be a maximum or a minimum—according to the kind of equilibrium (p. 172); and (3) that the whole distance of each body from the centers of forces need not be considered, but, if convenience of calculation requires it, we need only consider the distances of the bodies from fixed points on the lines of direction of the forces (pp. 172-173).

[15] Maupertuis does not add: "or a maximum." The subject of this memoir of 1740 and its connection with the principle of least action were afterwards greatly developed by Euler. Cf. also Mach, *Mechanik*, pp. 69-75; *Mechanics*, pp. 68-73.

[16] *Histoire de l'Académie*; *Année* 1744 (Paris, 1748), pp. 417-426; *Œuvres*, 1756, vol. iv, pp. 3-18 (with the addition referred to below).

pear[17] to be those of the reflection and of the refraction of light. When a ray of light in a uniform medium travels from one point to another, either without meeting an obstacle or with meeting a reflecting surface, nature leads it by the shortest path and in the shortest time. But when a ray is refracted by passing from a uniform medium to one of different density, the ray neither describes the shortest space nor does it take the shortest time about it. As Fermat showed, the time would be the shortest if light moved more quickly in rarer media, but Newton proved that, as Descartes had believed, light moves more quickly in denser media. Maupertuis's discovery was that light neither takes always the shortest path nor always that path which it describes in the shortest time, but *"that for which the quantity of action is the least."*

"I must now explain," he went one,[18] "what I mean by *the quantity of action*. A certain action is necessary for the carrying of a body from one point to another: this action depends on the velocity which the body has and the space which it describes; but it is neither the velocity nor the space taken separately. The quantity of action varies directly as the velocity and the length of path described; it is proportional to the sum of the spaces, each being multiplied by the velocity with which the body describes it. It is this quantity of action which is here the true expense (*dépense*) of nature, and which she economizes as much as possible in the motion of light."

Then Maupertuis found, as a consequence of his principle, that the sine of the angle of incidence is to the sine of the angle of refraction in the inverse ratio of the velocity of the light in each medium.[19] After showing that the law of reflection also follows from

[17] Maupertuis afterwards stated (see below, section V) that the agreement was between the laws of the motion of light and mechanical laws. I have given below my grounds for almost suspecting that this was not what Maupertuis originally meant.

[18] *Histoire de l'Académie*, 1744, p. 423; *Œuvres*, vol. iv, p. 17. Notice that *here*, in the *general* definition, *mass* is not mentioned. This is another reason for believing that, at first, Maupertuis only considered the motion of light-corpuscles, and not that of ordinary matter.

[19] Cf. Mach, *Mechanik*, pp. 397-398; *Mechanics*, pp. 367-368. Using Maupertuis's and Mach's figure, CRD is the horizontal refracting plane, AR is the incident and RB the refracted ray (A and B being any points chosen on these respective rays), m the velocity of light along AR and n the velocity along RB. Then Maupertuis says correctly that, according to his principle, $m.\mathrm{AR}+n.\mathrm{RB}$ must be a minimum. That is to say

$$d[m\sqrt{(\mathrm{AC}^2+\mathrm{CR}^2)}+n\sqrt{(\mathrm{BD}^2+\mathrm{DR}^2)}]=0,$$

whence, carrying out the differentiations, observing that AC and BD are constant, and $d(\mathrm{CR})=-d(\mathrm{DR})$,

$$(\mathrm{CR}/\mathrm{AR} : \mathrm{DR}/\mathrm{BR}) :: n : m, \text{ or } (\sin \mathrm{CAR}/\sin \mathrm{RBD}) = (n/m),$$

which is correct on the corpuscular hypothesis; Mach's criticism that the

his principle of the least quantity of action, Maupertuis concluded:[20] "We cannot doubt that all things are regulated by a supreme Being, who, while he has imprinted on matter forces which show his power, has destined it to execute effects which mark his wisdom;......" And:[21] "Let us calculate the motion of bodies, but let us also consult the designs of the Intelligence which makes them move."

It is of interest, in connection with the dispute with König which arose afterwards, to read the note which Maupertuis appended to the reprint in his *Œuvres*:[22]

"When I read the preceding memoir in the Paris Academy of Sciences, I only knew of what Leibniz had done on this matter by what M. de Mayran says of it in his memoir on the reflection of bodies in the Paris *Mémoires* for 1723. Like him, I had confused this opinion of Leibniz's with that of Fermat...."

Then he gave,[23] after Euler,[24] the full opinion of Leibniz.[25]

Now we shall see below that Maupertuis in the *Histoire* for 1752 said that he had "adopted" Leibniz's definition of *action*. We have no means of knowing how far, if at all, Maupertuis was indebted to the ideas of Leibniz.

IV.

There is nothing on the subject of the principle of the least quantity of action in the *Histoire de l'Académie de Berlin* (which contains the *Mémoires* of the various classes of the Academy) for 1745; but, in the *Histoire* for 1746, published in 1748, Maupertuis

reciprocal values appear instead of the actual ones is only true, as P. Stäckel observed in the *Encykl. der math. Wiss.*, vol. iv, part i, 1908, p. 491, on the undulatory theory, which Maupertuis, as a good Newtonian, did not adopt.

Further, Maupertuis's principle *does* state that $m.\mathrm{AR}+n.\mathrm{RB}$ (which is what $\int v.ds$ reduces to here) is to be a minimum. This was contested by Mach (but cf. *Mechanik*, p. 406; *Mechanics*, pp. 375-376).

Du Bois-Reymond (*op. cit*)., pp. 48-49) speaks of the example of the motion of light which Maupertuis chose in 1744 to illustrate his principle being "not happily chosen," because experiments have proved that the velocity of light in air is greater than that in water—the opposite state of things to that which the emission theory required.

[20] *Œuvres*, vol. iv, p. 21.

[21] *Ibid.*, p. 22.

[22] *Ibid.*, p. 23.

[23] *Ibid.*, pp. 23-28. In the text of the memoir of 1744, Maupertuis (*ibid.*, p. 15) thus mentioned Leibniz: "Leibniz wished to conciliate the opinion of Descartes [that light moves more quickly in the denser media] with final causes; but he did this only by suppositions which could not be sustained, and which did not square with the other phenomena of nature."

[24] *Hist. de l'Acad. de Berlin*, vol. vii, 1751, pp. 205-209.

[25] *Acta Eruditorum*, 1682 (not seen).

has[26] a memoir: "Les Loix du Mouvement et du Repos, déduites d'un Principe Métaphysique."

This memoir begins with the prefatory remark:[27] "I gave the principle on which the following work is founded on April 15th, 1744, in the public assembly of the Royal Academy of Sciences of Paris, as the *Acta* of this Academy testify." Then Maupertuis refers to Euler's *Methodus inveniendi* of 1744,[28] and the special pleasure that the Appendix gave him, "as," he says, rather patronizingly and in words which led some[29] to suppose that Euler merely applied Maupertuis's principle, "it is a beautiful application of my principle to the motion of the planets, of which this principle is in fact the rule."

The memoir is composed of three parts: (1) Examination of the proofs of the existence of God, which are drawn from the wonders of nature;[30] (2) The thesis that these proofs must be sought in the general laws of motion, and that the laws according to which motion is conserved, distributed, and destroyed are founded on the attributes of a supreme intelligence;[31] and (3) Investigation of the laws of motion and rest.[32] In the third part, Maupertuis[33] states the general principle that "when some change happens in nature, the quantity of action necessary for this change is the smallest possible," and adds: "*The quantity of action* is the product of the mass of the bodies by their velocity and by the space which they describe. When a body is transported from one place to another, the action is greater in proportion as the mass is greater, as the velocity is greater, and as the path by which it is transported is longer." From this principle, Maupertuis deduces the laws of impact of hard (or inelastic) and elastic bodies,[34] and of the lever.[35]

[26] Pp. 267-294. The mathematical (third) part of this memoir is, in part, identical with "Recherche des Loix du Mouvement" in the *Œuvres*, vol. iv, pp. 31-42; the theological part is included in the *Essai de Cosmologie* to which we will soon refer.

[27] *Histoire de l'Acad. de Berlin*, 1746, p. 267. This note was repeated in Maupertuis's *Œuvres*, vol. i (see below).

[28] See below, section IX.

[29] For example La Beaumelle, *op. cit.*, p. 85.

[30] *Histoire de l'Acad. de Berlin*, 1746, pp. 268-277.

[31] *Ibid.*, pp. 277-287.

[32] *Ibid.*, pp. 287-294.

[33] *Ibid.*, p. 290; *Œuvres*, vol. iv, p. 36.

[34] *Histoire*, pp. 290-293; *Œuvres*, vol. iv, pp. 36-42.

[35] *Histoire*, p. 294; not in the *Œuvres*. The explanation of this omission given by Maupertuis (*Œuvres*, vol. i, p. xxvii) is that this problem is too limited (as the directions of the forces of weight are all supposed to be parallel to one another and at right angles to the straight lever); but the "Loi du

When treating of impact of hard (inelastic) bodies of masses A and B, which move with the velocities a and b respectively in a straight line and in the same sense, Maupertuis considers the spaces (a and b) *described in a certain time* (the unit of time), so that $m.v.s$ becomes $m.v^2$, as Mach notices, and so he points out Maupertuis's inconsistency.[36]

Let A move faster than B, so that A catches B up and infringes on it, and let the common velocity of A and B after the impact be x (less than a and greater than b). "The alteration which has happened in the universe consists in that the body A which moved with the velocity a and which in a certain time described a space equal to a only moves with the velocity a and describes a space equal to x, while the body B which only moved with the velocity b and described a space equal to b moves with a velocity x and describes a space equal to x. This change is, then, the same as would have happened if, while A moved with the velocity a and described a space equal to a, it had been carried backwards through a space equal to $a-x$ on an immaterial plane moving with the velocity $a-x$, and while B moved with the velocity b and described a space equal to b, it had been carried forward through a space equal to $x-b$ on an immaterial plane moving with a velocity $x-b$. Now, whether A and B move with their own velocities on movable planes or they are at rest there, as the movement of these planes charged with bodies is the same, the quantities of action produced in nature will be $A(a-x)^2$ and $B(x-b)^2$, and their sum must be as small as possible." This gives

$$2.A.a.dx + 2.A.x.dx + 2.B.x.dx - 2B.b.dx = 0,$$

whence

$$x = (Aa + Bb)/(A+B).$$

In this case, where the bodies move in the same direction, the quantity of motion destroyed and the quantity produced are equal, and the total quantity of motion remains, after the impact, the same as it was before. If the bodies move towards one another it is easy to apply the same reasoning; or it is sufficient to consider b as negative with respect to a. Then the common velocity will be

$$x = (Aa - Bb)/(A+B).$$

If A and B are perfectly elastic, and move in the same direction with velocities as before, except that α and β are the respective

repos" of 1740, given in vol. iv of the *Œuvres*, is a *general* principle of statics and "agrees so perfectly with the principle of the least quantity of action that we may say that it is only the same thing."

[36] *Mechanik*, pp. 395-396, 398; *Mechanics*, pp. 365-366, 368.

velocities after impact, "the sum or the difference of these velocities after the impact being the same as it was before," then, by analogous considerations on the change which has happened in nature, Maupertuis arrives at the conclusion that the quantity of action is here

$$A(a-\alpha)^2+B(b-\beta)^2$$

and this, when minimized, since

$$\beta-\alpha=a-b \text{ and thus } d\beta=d\alpha,$$

gives

$$\alpha=(Aa-Ba-2Bb)/(A+B), \qquad \beta=(2Aa+Ab-Bb)/(A+B).$$

Here the sum of the *vires vivae* is conserved on impact, but this is not the case with hard (inelastic) bodies.

To find the law of the lever Maupertuis considers masses A and B attached to the ends of an immaterial lever of length c, and seeks the point, at a distance z from A, around which they are in equilibrium. For this purpose he seeks the point around which, if the lever receives some small movement, the quantity of action is the smallest possible. Then A and B, on this movement being imparted to them, describe small arcs similar to one another and proportional to the distances of these bodies from the point sought. These arcs will be the spaces described by the bodies and at the same time will represent their velocities. Thus the quantity of action will be proportional to

$$Az^2+B(c-z)^2$$

and this, when minimized, gives

$$z=Bc/A+B.$$

V.

In the "Avertissement" to the fourth volume of his *Œuvres*, Maupertuis says of the memoir of 1744: "I show the agreement of the laws which light follows in its reflection and its refraction with those which all other bodies follow in their motion." In point of fact, this is not quite the case: he shows how *both* the law of reflection and that of refraction could, on the corpuscular hypothesis, be deduced from one principle; but, in the whole memoir, other motions than that of light were only referred to shortly. The law that, in a uniform medium, light moves in a straight line is common, he says,[37] to all bodies: they move in a straight line unless some external force deflects them; and the law of reflection is the same as that

[37] *Paris Histoire*, 1744, p. 418; *Œuvres*, vol. iv, p. 7.

followed by an elastic ball impinging on an unbreakable surface. But no like explanation of the law of refraction had been given.

Later on, Maupertuis[38] adds a note to his definition of the quantity of action as $\Sigma s.v$: "As here there is only one body, we abstract from its mass."

VI.

Maupertuis's *Essai de Cosmologie* was published in 1751,[39] and consists of three parts: (1) Examination of the proofs of the existence of God, which are drawn from the wonders of nature; (2) Deduction of the laws of motion from the attributes of the supreme intelligence; and (3) Spectacle of the universe. No part of the work is stated mathematically, and the third part is a rhetorical sketch of the solar system, in which the principle of the least quantity of action is not mentioned.[40] The two first parts are practically the two first parts of the memoir of 1746.

[38] *Œuvres,* vol. iv, p. 17. This note is not in the original memoir of 1744 (the paragraph in the text to which the note refers is on p. 423 of this memoir), but was first added, as a marginal note, in the *Essai de Cosmologie* of 1751. These facts suggest that the mechanical applications of Maupertuis's principle were, at least, not clear to Maupertuis in 1744. For my own part, I cannot help almost having the impression from a study of the original memoir of 1744 and its reproduction, with comments, in the *Œuvres* of 1756, that the laws of nature referred to in 1744 are the laws of catoptrics and dioptrics, whereas afterwards Maupertuis, because of the discovery communicated in his memoir of 1746, tried to persuade possibly himself and certainly his readers that the laws were more general laws of nature. Cf. Note 18, Section III, above.

Formey, in the *Eloge* quoted at the beginning of this paper, says (p. 496): "Il y [in the memoir of 1744] étoit principalement question des loix qui suit la lumière, surtout lorsqu'elle passe d'un milieu diaphane dans un autre."

[39] *Essay de Cosmologie.* Par M. de Maupertuis, Leyden, 1751. At the end (pp. 81-104) is a reprint of the 1744 paper with the mathematics (the note referred to in section V, last note, is put in the margin of pp. 97-98); and on pp. 63-80 is a "Recherche mathématique des Loix du Mouvement et du Repos," from, says Maupertuis, the Berlin *Mémoires* for 1747 (a misprint for 1746). The *Essai* was partly reprinted in the first volume of the *Œuvres de Mr. de Maupertuis* (Nouvelle édition, corrigée et augmentée, Lyons, 1756, pp. 3-78, and the mathematical part, which was omitted in the previous editions of Maupertuis's *Œuvres,* is included in vol. iv, pp. 18-19, 36-42. On pp. iii-xxviii, is an "avant-propos" giving, among other things, an account of the Koenig incident of 1751 and its consequences. On pp. xiv-xv is the same notice about his own and Euler's works of 1744 that is at the head of Maupertuis's paper in the Berlin *Mémoires* for 1746. On d'Arcy's objections (see section XV) Maupertuis (*Œuvres,* vol. i, p. xxvi) said that "As the only objection which appears to have some foundation rests on the fact that, in the impact of elastic bodies, he has confused the change which happens to the velocities (which is real) with the change of the quantity of action (which is zero), I will make no other reply than the few words I have said about it in the *Mémoires* of our [Berlin] Academy for the year 1752" (see section XVI).

[40] However, in the second part (*Œuvres,* vol. i, p. 45), we read: "What a satisfaction for the human mind to find in the laws which are the principle

Maupertuis had a low opinion of the proofs of the existence of God from the construction of animals. Thus, somebody[41] found evidence for this existence in the folds of the skin of a rhinoceros—the animal could not move without these folds. Maupertuis[42] rather appositely asked: "What would be said of a man who should deny a Providence because the shell of a tortoise has neither folds nor joints?" And:[43] "It is not in the little details, in those parts of the universe of whose relations are known too little, that we must look for the supreme Being, but in phenomena whose universality suffers no exception and whose simplicity lays them quite open to our sight."

VII.

The reason why Maupertuis laid stress on the deduction from the principle of the least quantity of action of the laws of the impact of inelastic masses was that the law of the conservation of *vis viva* fails in this case.[44] Leibniz[45] recognized Descartes's error in thinking that, in nature, the sum of the products of the masses into their respective velocities is constant, and substituted in it the squares of the velocities for the velocities, so that the sum is what is called the *vis viva* of the system considered. But, in impact, the *vis viva* is only conserved if the bodies are elastic; and, according to Maupertuis:[46] "When we make this objection to the Leibnizians, they prefer to say that there are no hard (*durs*, inelastic) bodies in nature than to abandon their principle. This were to be reduced to the strangest paradox to which love of a system could reduce one: for what can the primitive elementary bodies be but hard bodies?"

In vain, then, said Maupertuis,[47] did Descartes and Leibniz, in

of motion of all the bodies of the universe the proof of the existence of the governor of it!"

[41] *Phil. Trans.*, No. 470. [The paper referred to is entitled: "A Letter from Dr. Parsons to Martin Folkes, Esq., President of the Roy. Soc., containing the Natural History of the Rhinoceros," and is printed in the *Phil. Trans.* for 1743, pp. 523-541].

[42] *Œuvres*, vol. i, p. 12.

[43] *Ibid.*, p. 21.

[44] *Œuvres*, vol. i, pp. xvi-xvii, 44.

[45] On Leibniz's mechanics (the conservation of *vis viva*, and so on), cf. Bertrand Russell, *A Critical Exposition of the Philosophy of Leibniz, with an Appendix of leading Passages*, Cambridge, 1900, pp. 77-99, 226-238; esp. pp. 89-90. The concept of *action* with Leibniz was not mentioned by Russell; on it cf. du Bois-Reymond, *op. cit.*, pp. 48, 51, 89-90; and Helmholtz, "Zur Geschichte des Princips der kleinsten Action," *Sitzungsberichte der Berliner Akad.*, 1887, pp. 225-236, or *Wiss. Abh.*, vol. iii, pp. 249-263. Cf. also L. Couturat, *La logique de Leibniz*, 1901, pp. 229-233, 577-581.

[46] *Op. cit.*, p. xvii.

[47] *Ibid.*, p. xviii.

different ways, imagine a world which could dispense with the hand of a Creator: no quantity which can be regarded as a cause in the distribution of motion subsists unaltered. But "Action" is, so to speak, created at each instant, and always created with the greatest economy possible; and by this the universe announces its dependence on a wise and powerful author.

Maupertuis[48] said that, because he held that the conservation of *vis viva* is not the universal principle of movement, the whole sect of Leibnizians in Germany descended on him (*je vis fondre sur moi toute la secte que M. de Leybnitz a laissée en Allemagne*), and then mentioned[49] König's having attributed some of Maupertuis's and Euler's discoveries to Leibniz. Then follows[50] an account of the incident.

As a justification of the word "action," Maupertuis[51] remarked that he had found this word quite established by Leibniz and Wolff, and did not wish to change the terms.

VIII.

When speaking of Diderot's *Thoughts on the Interpretation of Nature* of 1754, John Morley,[52] now Lord Morley, said:

"Maupertuis had in 1751, under the assumed name of Baumann, an imaginary doctor of Erlangen, published a dissertation on the *Universal System of Nature,* in which he seems to have maintained that the mechanism of the universe is one and the same throughout, modifying itself, or being modified by some vital element within, in an infinity of diverse ways.[53] Leibnitz's famous idea, of making nature invariably work with the minimum of action, was seized by Maupertuis, expressed as the Law of Thrift, and made the starting point of speculations that led directly to Holbach and the *System of Nature.*[54] The *Loi d'Epargne* evidently tended to make unity

[48] *Ibid.,* p. xix.

[49] *Ibid.,* p. xx.

[50] *Ibid.,* pp. xx-xxvi, cf. section XI below.

[51] *Ibid.,* pp. xxvi-xxvii, cf. Maupertuis's paper of 1752, described below in section XVI.

[52] *Diderot and the Encyclopædists,* vol. ii, London, edition of 1905, pp. 262-263.

[53] "As to the precise drift of Maupertuis's theme, see Lange, *Gesch. d. Materialismus,* i, 413, *n.* 37. Also Rosenkranz, *Diderot's Leben,* 1866, vol.i, p. 134."

[54] "In 1765 Grimm describes the principle of Leibnitz and Maupertuis as 'gaining on us on every side'....*Corr. Lit.,* iv, 186." [Under the date of Feb. 15, 1765, Grimm (*Correspondance littéraire philosophique et critique de Grimm et de Diderot depuis 1753 jusqu'en 1790,* new ed., vol. iv, p. 186) speaks thus

of all the forces of the universe the keynote or the goal of philosophical inquiry. At this time of his life, Diderot resisted Maupertuis's theory of the unity of vital force in the universe, or perhaps we should rather say that he saw how open it was to criticism. His resistance has none of his usual air of vehement conviction. However that may be, the theory excited his interest, and fitted in with the train of meditation which his thoughts about the Encyclopaedia had already set in motion, and of which the *Pensées Philosophiques* of 1746 were the cruder prelude."

Again:[55]

"Diderot was in no sense the originator of the French materialism of the eighteenth century. He was preceded by Maupertuis, by Robinet, and by La Mettrie; and we have already seen that when he composed the *Thoughts on the Interpretation of Nature* (1754) he did not fully accept Maupertuis's materialistic thesis. Lange has shown that at a very early period in the movement the most consistent materialism was ready and developed, while such leaders of the movement as Voltaire and Diderot still leaned either on deism and scepticism."[56]

Lange's[57] work was first published in one volume: *Geschichte des Materialismus und Kritik seiner Bedeutung in der Gegenwart* at Iserlohn in 1866. In the whole book, Maupertuis is only mentioned once. On page 224[58] it is said that people debated whether the Marquis d'Argens (Jean Baptiste de Boyer) or Maupertuis or some personal enemy of Albrecht von Haller, really wrote the *Homme machine* which De la Mettrie ironically dedicated to Von Haller.[59]

The fourth part[60] is devoted to the materialism of the eighteenth century, and consists of three divisions: De la Mettrie's *Homme machine* of 1747;[61] Holbach's *Système de la Nature, ou des lois du monde physique et du monde moral* of 1770, published, according to the title-page, in London, but really at Amsterdam, under the

of the Leibniz-Maupertuis principle of thrift, immediately after speaking of the second volume of Robinet's *De la nature,* published in four volumes 1761-8.

On Holbach's *System of Nature* (1770), see Morley, *op. cit.,* pp. 155-203.

[55] Morley, *op. cit.,* pp. 272-273.

[56] *Gesch. d. Materialismus,* i, 309, 310, etc.

[57] Friedrich Albert Lange.

[58] Cf. the references below the second edition of Lange's work.

[59] Lange, *op. cit.,* p. 72.

[60] *Ibid.,* pp. 163-229.

[61] *Ibid.,* pp. 163-186.

name of Mirabaud who had been dead for ten years;[62] and the German reaction against materialism.[63]

On the other hand, Maupertuis is often spoken of in the second edition of Lange's work, published at Iserlohn in 1873 and 1875 in two volumes under the same title,[64] and it is to this edition that Morley's citations refer. We will continue this reference to Lange's book after having given some information about Maupertuis's work of 1751, which Morley mentions.

In 1751 Maupertuis published at Erlangen, under the pseudonym of "Baumann," a Latin dissertation under the title: *Dissertatio inauguralis metaphysica, de universali naturae systemata*,[65] in which

[62] *Ibid.*, pp. 186-214.

[63] *Ibid.*, pp. 214-229.

[64] There is an English translation of this edition in three volumes, by E. C. Thomas, published at London in 1877, 1880 and 1881 (*History of Materialism and Criticism of its Present Importance*). The passages in this translation parallel to those of Morley's citations are given here.

[65] Another edition, with a French translation and with neither the place nor year of publication has been given; a third, only in French and entitled: *Essai sur la formation des corps organisées* was published by l'Abbé Trublet, with a notice and conjectures about the author, at Berlin (really at Paris) in 1754; and the French version (*Système de la Nature*: *Essai sur la formation des corps organisées*) was published, with a preface, in Maupertuis's *Œuvres*, 1756, vol. ii, pp. 135-168 (between pp. 160 and 161 are pages numbered 145* to *160). Diderot's *Pensées sur l'interprétation de la nature* was published anonymously at Paris in 1754 with "London" as the place of printing (Cf. Karl Rosenkranz, *Diderot's Leben und Werke*, 2 vols., Leipsic, 1866, vol. i, pp. 134-146; *Œuvres complètes de Diderot*, ed. by J. Assézat, vol. ii, Paris, 1875, pp. 1-63; cf. Assézat's "Notice préliminaire," p. 3. Maupertuis's "Réponse aux objections de M. Diderot," was printed in his *Œuvres*, 1756, vol. ii, pp. 169-184 (between pp. 176 and 177 are pages numbered 161* to *176). Cf. on all this, La Beaumelle, *op. cit.*, pp. 178-181, 200-201.

On Maupertuis's theories of generation, see La Beaumelle, *op. cit.*, pp. 86-87, 98-103; du Bois-Reymond, *op. cit.*, pp. 38-39, 44-45. The *Vénus physique* of 1745 (anonymous) was republished in Maupertuis's *Œuvres*, 1756, vol. ii, pp. 1-133. The statement that Maupertuis endeavored to explain the formation of the foetus by gravitation is one of Voltaire's libels on Maupertuis. The truth seems to be that Maupertuis, in his *Vénus* and *Système de la Nature*, as well as in one of his *Letters* ("Lettre xiv, Sur la génération des animaux," *Œuvres*, 1756, vol. ii, pp. 267-282), tried to explain this formation by the different *attractions* or (in the *Système*) psychical tendencies of the different parts. The *Lettres de M. de Maupertuis* (*sur différents sujets*) were published in 1753 and again in the *Œuvres*, 1756, vol. ii, pp. 185-340, after having been grossly caricatured by Voltaire in his *Histoire du docteur Akakia et du natif de Saint Malo* (*Œuvres complètes de Voltaire*, vol. xxiv, Paris, 1892, pp. 358-376). By the way, Letters X and XI ("Sur les loix du mouvement" and "Sur ce qui s'est passé à l'occasion du principe de la moindre quantité de l'action"; *Œuvres*, 1756, vol. ii, pp. 238-242 and 243-251 respectively) refer to the principle of least action; and Letter XII (*ibid.*, pp. 252-257; "Sur l'attraction") contains a short *exposé* of Maupertuis's work in introducing Newtonianism into France.

Maupertuis does not seem, by his published writings, to have been nearly so ridiculous a person as Voltaire, for personal reasons, tried to make him appear to be. And Voltaire's sarcasms have had great influence on the ideas

a hypothesis that the parts of matter have something similar to what we call desire, aversion, and memory was advanced to explain certain physiological facts. Maupertuis chose this pseudonymous fashion of giving his thoughts to the public, partly because the work of an unknown author would be less the butt of objections, and partly in order that he should not be obliged to reply to them. But he felt it necessary to reply to Diderot's *Thoughts* because his doctrines were accused of having results contrary to religion. Then he acknowledged the work: he had soon been recognized as its author. What concerns us here is that the law of least action is not mentioned in this work of Maupertuis's. Further, the *Essai de Cosmologie* of 1751 was not published anonymously or pseudonymously. Thus there seem to be no grounds for Morley's strange error.

Lange shows that the Newtonian theory is a combination of materialism in natural science with a religious faith in the spiritual constructor of the material world-machine. "The magnificent phenomena of the seventeenth century were renewed in increased splendor, and to the age of a Pascal and Fermat succeeded with Maupertuis and D'Alembert the long series of French mathematicians of the eighteenth century, until Laplace drew the last consequences of the Newtonian cosmology in discarding even the hypothesis of a creator."[66]

Maupertuis is classed with Robinet and La Mettrie as a materialist[67] on the grounds of his Latin dissertation of 1751. The English translation of the note (37) referred to by Morley is:[68] "*Comp.* Rosenkranz, *Diderot,* i, 134 ff. The pseudonymous dissertation of Dr. Baumann (Maupertuis) I have not seen, and it may be open to some doubt, according to Diderot and Rosenkranz, whether it does really contain the materialism of Robinet—that is, the unconditional dependence of the spiritual upon the purely mechanical series of external events—or whether it inculcates Hylo-

of Maupertuis formed by succeeding generations. Thus Mach (*Mechanik,* pp. 484-485, *Mechanics,* pp. 454-455) gives, I think, Voltaire's version of some of the things dealt with by Maupertuis in a *Letter* published earlier than those just mentioned. Maupertuis's *Lettre sur les progrès des sciences* was published at Berlin in 1752 and again in his *Œuvres,* 1756, vol. ii, pp. 341-399. Here is the project of founding a town where only Latin should be spoken, in order to preserve this most universal of languages (pp. 367-368), and a plea (pp. 394-398) for "metaphysical"—or, as we would say now, psychological—experiments.

[66] Lange, *Geschichte,* 2d ed., vol. i, p. 304; *History,* vol. ii, p. 16.

[67] Lange, *Geschichte,* vol. i, p. 310; *History,* vol. ii, p. 25.

[68] Lange, *Geschichte,* vol. i, pp. 315, 412-413; *History,* vol. ii, p. 31.

zoism—that is, modifications of the natural mechanism by the spiritual content of nature according to other than purely mechanical laws."

Again:[69] "Buffon began the publication of his great work on natural history in the year 1749, with the first three volumes; but it was only in the fourth volume that he unfolded the idea of the unity of principle in the multiplicity of organisms, an idea which occurs again in Maupertuis in an anonymous work in 1751, in Diderot in the *Pensées sur l'Interprétations de la Nature,* 1754, while we find it developed with great clearness and distinctness by La Mettrie as early as the *L'Homme Plante* in 1748."

This, together with the passage referred to above, when we were speaking of the first edition, about Maupertuis being considered by some to be the author of *L'Homme Machine*,[70] completes the list of Lange's references to Maupertuis in the second edition of his book.

We must add that Maupertuis, in his writings and in his life, showed the greatest respect for religion. He was by no means a materialist and atheist,[71] and the only reason, said he, that he had for replying to Diderot's *Thoughts* on his dissertation of 1751 was that Diderot stated that the dissertation, in spite of its carefully religious tone, led to conclusions which were subversive of religion.

IX.

This seems the best place to give some account of the work of a man who will now take a prominent place in the development of Maupertuis's ideas; I mean Leonhard Euler.[72]

The modern period of the discussion of maximal and minimal problems begins with Johann Bernoulli's proposal of the problem of the brachistochrone in 1696 and the consequent rise into importance of the "isoperimetrical" problems.[73] The period 1696 to 1762 of

[69] Lange, *Geschichte,* vol. i, p. 328; *History,* vol. ii, p. 52.

[70] Lange, *Geschichte,* vol. i, p. 398; *History,* vol. ii, p. 137.

[71] Du Bois-Reymond, *op. cit.,* pp. 43-44, 49-50.

[72] On the older period of the history of such problems, see Mach, *Mechanik,* pp. 453-457; *Mechanics,* pp. 421-425. This period is—like all early periods in the history of branches of science—characterized by the fact that the maximal and minimal problems are all *isolated.* This period extends as far as Newton who in 1687 solved "the first problem of the calculus of variations," the determination of the figure of the solid of least resistance (cf. M. Cantor, *op. cit.,* p. 291).

[73] Mach, *Mechanik,* pp. 457-467; *Mechanics,* pp. 425-436. A German annotated translation of some works of Johann Bernoulli, Jakob Bernoulli, and Leonhard Euler, from 1696 to 1744, is given by P. Stäckel in No. 46 of *Ostwalds Klassiker.* Cf. also M. Cantor, *op. cit.,* pp. 237-241, 384, 446-458, 533, 846-848.

the history of such problems is distinguished by the names of Johann Bernoulli, Jakob Bernoulli, and Leonhard Euler, and extends until Lagrange, in 1762, brought all these interrelated methods under the general and abstract analytical form of the calculus of variations. It is to this period that the works of Maupertuis, Euler, and their contemporaries, with which we are concerned here, belong. The leading work published in this period was the famous *Methodus inveniendi lineas curvas maximi minimive proprietate gandentes: sive solutio problematis isoperimetrici latissimo sensu accepti* which was published at Lausanne and Geneva in 1744.[74]

Mathematicians found that various problems of mechanics might be put into isoperimetrical form. Whether their tendency to do this, which was very common at that time, was due to esthetic, theological, or technical reasons, it is hard to say. Daniel Bernoulli —a son of Johann Bernoulli—remarked that certain statical problems can be treated with greater facility by isoperimetrical methods than by the usual mechanical principles; the feeling, too, that the discovery that a problem about natural objects could be put in a maximal or minimal form had a connection with the way the Deity managed things here below in making nature act by the shortest or easiest or readiest paths, and so with what were then called "metaphysical"[75] questions, undoubtedly had an influence on others besides Maupertuis—on Euler for example. But we shall see how piety and humility led Euler, though accurate, judged by the mathematical standards of those days, very cautious, and perhaps a little unimaginative,[76] to accept and admire the bold and not always accurate mechanical generalizations which Maupertuis professed to deduce from "metaphysics." But probably the esthetic satisfaction which

[74] An annotated German translation of a great part of this book was given in No. 46 of *Ostwalds Klassiker.* However, the two appendices (on the elastic curves, and on the motion of a particle round a center of force in a non-resisting medium) with which we shall be especially concerned here were not translated with the main body of the work. But the first appendix was translated, in another connection, in No. 175 of the *Klassiker* (see below, section X). An account of Euler's book of 1744 is given in M. Cantor's *Geschichte,* vol. iii, 2d ed., Leipsic, 1901, pp. 857-867.

[75] In the eighteenth century, "metaphysics" stood for—at least among mathematicians—a branch of learning which included theology, psychology, and logic. Consider the "metaphysical experiments" advocated by Maupertuis, and the "metaphysics of the infinitesimal calculus" (L. N. M. Carnot, Lagrange, and others), which meant what we mean when we say: "the logical principles of the calculus."

[76] D'Alembert, in a letter of March 3, 1766, to Voltaire (quoted by Delambre in his "Notice" in *Œuvres de Lagrange,* vol. i, p. xxi), says of Euler: "c'est un homme peu amusant, mais un très grand géomètre."

arises from stating a problem in a maximal or minimal form influenced mathematicians the most.

However this may be, to this form come many problems of statics, such as the catenary of Johann and Jakob Bernoulli,[77] and Jakob Bernoulli's problem of the elastic curve.[78] From Daniel Bernoulli's letter to Euler and from Euler's first appendix to his book of 1744, we see with what interest Daniel Bernoulli and Euler reduced this problem in the theory of elasticity to isoperimetrical methods.

These problems were all *statical* ones; and it was Daniel Bernoulli who suggested to Euler the putting of a certain *dynamical* problem into isoperimetrical form. It must be remembered that Euler, by his papers published by the St. Petersburg Academy in 1732 and 1736,[79] had placed himself at the head of the mathematical world, in the treatment of isoperimetrical problems. We must now say some words about Daniel Bernoulli and Euler and their relations to one another.

Daniel Bernoulli[80] (1700-1782) was a son of the famous Johann Bernoulli (1667-1748) and was attached to the St. Petersburg Academy from 1725 to 1733. From 1733 to 1782 he was Professor of Anatomy and Botany, and later Experimental Physics and Speculative Philosophy too, at Basel. His mathematical works[81] are on differential equations, the theory of numbers, the theory of probability, series, and mechanics[82]—principally the theorem of *vis viva*,[83] the problem of vibrating cords,[84] and hydrodynamics.[85] Leonhard Euler[86] (1707-1783), whose name as a mathematician is too well known for it to be necessary for us to refer further to his many works, came to St. Petersburg in 1727, owing to the exertions on his behalf of Daniel Bernoulli and Hermann, but left St. Peters-

[77] Cf. Mach, *Mechanik*, pp. 75-77; *Mechanics*, pp. 74-76; *Ostwalds Klassiker*, No. 46, p. 19; M. Cantor, *op. cit.*, pp. 219-220, 228, 235, 289, 384, 455, 853.

[78] Cf. M. Cantor, *op. cit.*, pp. 220-221, and Johann Bernoulli's letter of March 7, 1739, to Euler in Fuss's *Correspondance* referred to below, vol. ii, pp. 23-25.

[79] Cf. M. Cantor, *op. cit.*, pp. 846-856.

[80] M. Cantor, *op. cit.*, pp. 89-90, 550; *Encycl. Brit.*, 9th ed., vol. iii, 1875, pp. 606-607.

[81] *Ibid.*, pp. 477-481, 610, 630-632, 634-635, 640, 642-644, 688, 693, 707, 721, 851, 900, 904-906.

[82] Cf. also Mach, *Mechanik*, pp. 43-49, 326; *Mechanics*, pp. 40-47, 293.

[83] Cf. also Mach, *Mechanik*, pp. 374-379; *Mechanics*, pp. 343, 348.

[84] Cf. also Mach, *Die Principien der Wärmelehre*, 2d ed., Leipsic, 1900, pp. 96-97.

[85] Cf. Mach, *Mechanik*, pp. 440-453; *Mechanics*, pp. 403-420.

[86] M. Cantor, *op. cit.*, pp. 540-551

burg in 1744 to become Director of the Mathematical Class of Frederick the Great's reformed Academy of Sciences at Berlin. In 1727 Euler met Daniel Bernoulli and was stimulated by him to an investigation on geodesic lines.[87] The letters addressed by Daniel Bernoulli to Euler—those from Euler to Bernoulli are unfortunately lost— from 1726 to 1755 have been published in P. H. Fuss's *Correspondance mathématique et physique de quelques célèbres géomètres du XVIIIième siècle.*[88] From this correspondence we will now make the extracts which concern our present subject.

In a letter to Euler of January 28th, 1741, Daniel Bernoulli asked whether it was not Euler's opinion that orbits about centers of force could be deduced by an isoperimetrical method.[89] As we have said, Euler's replies are lost. In a letter of December 12, 1742, Bernoulli has some further remarks on the same subject;[90] and in a

[87] M. Cantor, *op. cit.*, p. 843.

[88] St. Petersburg, 1843, vol. ii, pp. 407-655. In these letters there is frequently mention of isoperimetrical problems, but the first mention of a *mechanical* problem treated by an isoperimetrical method is on pp. 456-457 (letter of March 7, 1739) where the elastic curve, which requires a certain integral which represents the *"potential vis viva"* to be a maximum, since Bernoulli thinks "that an elastic lamina which takes a certain curvature of itself will bend in such a way that the *vis viva* will be a minimum, since otherwise the lamina would move," is referred to (other references are given on pp. 468-469, 506-507, 512-514, 533-534, 536-537). To this apparently refers what Bernoulli (p. 534) calls an *a priori* method—a speculation which contrasts oddly with the passages quoted below which are rather anti-"metaphysical." The first occurrence of a reference to a *dynamical* problem to be treated by an isoperimetrical method is that given below.

It was Daniel Bernoulli who recommended that Bousquet of Geneva should be chosen as the printer of Euler's "masterly" (*herrlichen*) treatise on the isoperimetrical method—the *Methodus* printed in 1744 (letter of Feb. 9, 1743; *ibid.*, p. 521; cf. pp. 524-525 (see extract below), 528, 529, 533 (see extract below), 541, 550, 553, 578). In a letter of September 4, 1743, Bernoulli (*ibid.*, p. 536) says: "I regret that I could not read through your additions to the treatise on isoperimeters; but I have just (*fugitivo oculo*) glanced at them." This is important in view of Euler's account (section XII below) of the date and circumstances under which these additions were made and printed.

[89] "Von Ew. möchte vernehmen, ob Sie nicht meinen, dass man die orbitas circa centra virium könne methodo isoperimetrica, wie auch die figuram terrae pro theoria Newtoniana herausbringen" (Fuss, *Correspondance*, vol. ii, p. 468).

[90] "Man kann die principia maximorum et minimorum nicht genugsam ausforschen; die trajectoriae circa centrum virium, vel circa plura centra virium, müssen gleichfalls per methodum isoperimetricorum können solviret werden, obschon man das maximum vel minimum, quod natura affectat, nicht einsiehet. Es haben also Ew. einen grossen Nutzen dadurch geschafft, dass Sie die methodum isoperimetricorum so weit perfectionnirt haben. Meiner Meinung nach ist dieses argumentum inter omnia pure analytica utilissimum, und ist dieses ein wahres Exempel, dass vel sola propositio problematis, wenn man auch die Solution nicht hätte, saepe maxima laude digna sey" (*ibid.*, p. 513).

letter of April 23, 1743, speaks[91] with praise of Euler's great treatise on the Isoperimetrical Method, suggests the addition of a treatment of the problem of the elastic curve and others like it, and then comments on Euler's discovery that $\int v.ds$ is a minimum for central orbits, that Euler has obviously communicated to him without proof, as follows:

"The observation about trajectories that $\int v.ds$ must be a maximum or minimum appears to me very beautiful and important; but I cannot see how this principle is demonstrated. Please let me know whether the principle extends to trajectories about many centers of forces. Perhaps it is only an observation *a posteriori,* owing to a discovery you may have made that the trajectories have this property, and you may not have been able to demonstrate it *a priori.*"

In a letter of September 4, 1743, Bernoulli writes:[92]

[91] Wegen Ew. herrlichen Tractat de isoperimetricis werde ich vorläufig mit demselben reden; Sie belieben nur denselben fertig zu halten. Sie könnten das problema de elastica hac methodo invenienda und andere dergleichen noch beyfügen. Ich sehe leicht, dass man die curvaturam catenae et laminae elasticae oscillantis auch darin reduciren kann; auf den modum aber bin ich noch nicht bedacht gewesen. Die meisten curvas mechanicas wird man auch dahin reduciren können. Die Observation von den trajectoriis, dass $\int v.ds$ ein maximum oder minimum seyn müsse, dünkt mich sehr schön und von grosser Wichtigkeit; ich sehe aber die Demonstration dieses principii nicht ein. Ew. belieben mir zu melden, ob sich solches auch ad trajectorias circa plura centra virium erstrecke. Vielleicht ist es nur eine observatio a posteriori, indem Sie angemerkt haben, dass die trajectoriae diese proprietatem haben, ohne solche a priori recht demonstriren zu können" (*ibid.,* pp. 524-525).

[92] "Aus Dero Brief ersehe ich, dass ich in meiner Conjectur mich nicht betrogen, wenn ich gesagt habe, dass Dero Observation circa orbitas planetarum, in quibus $\int v.ds$ vel $\int v.v.dt$ ein minimum ist, vielleicht nur a posteriori sey gemacht worden; denn nach meinen principiis kann ich solches a priori nicht einsehen. Der Herr Clairaut schreibt, dass solches auch schon von einem Engländer sey remarquirt worden. Es scheint, dass dieses nicht sowohl ein principium, als eine proprietas sey, gleich wie es eine proprietas ist elasticae, dass sie das maximum solidum generirt. Doch hab ich nicht untersucht, ob die idea maximi solidi die elasticam in omni extensione begreife. Sie können mich dieser Mühe entheben, denn ich weiss, dass Sie alle dergleichen Untersuchungen allbereits gemacht haben. Von meinem principio a priori, dass die elastica das $\int ds/rr$ ein minimum formire, hab ich mit vieler Erkenntlichkeit ersehen, aber zugleich mit Beschämung, dass Sie in Ihrem supplemento so honorificam mentionem thun. Dieses principium gehet auch an in laminis inaequaliter elasticis, wenn man macht $\int eds/r.r$ ein minimum. Die laminae naturaliter non rectae erfordern zwar einen andern calculum, aber keine andere methodum; wenn aber die laminae proprio pondere zugleich incurvirt werden, so ist es schwer, das maximum oder minimum quod natura affectat zu determiniren. Ich muthmaasse, dass man allhier muss ad maxima maximorum recurriren, wenn zweyerley Considerationen zusammen kommen. Quaeatur brevitatis gratia curva AC, quam lamina naturaliter recta AB et uniformis proprio solo pondere incurvata accipiet: fragt sich, ob nicht curva AC talis seyn könnte, dass inter omnes ejusdem longitudinis, inter eosdemque terminos positas curvas, eandemque $\int ds/rr$ habentes, das centrum gravitatis infimum locum obtineat. Wir haben Beide diese curvam directe determinirt; fragt sich also, ob man ex hoc principio eandem curvam finden

"From your letter I see that I was not mistaken in my conjecture that your observation that $\int v.ds$ or $\int v.v.dt$ is a minimum for the orbits of the planets was perhaps only made *a posteriori*; for I cannot see this *a priori* by the light of my principles. M. Clairaut writes that this property has also been noticed by an Englishman. It appears that this is not so much a principle as a property, just as it is a property of the elastic curve to generate the maximum solid. Still I have not investigated whether the idea of the maximum solid includes that of the elastic curve in all its extension...."

And in a letter of December 25, 1743, Bernoulli writes:[93]

"I doubt whether one can ever show *a priori* that the elastic curve must generate the maximum solid; I consider this as a property which is shown by calculation and that nobody could have foreseen from first principles—as little as the identity of the isochrone and the brachystochrone. Such properties are, as it were, discovered through accident by our reason, and I consider the property observed, that in orbits $\int u.ds$ is a minimum, to be on this level. I was confirmed in this opinion by learning that you only observed this property *a posteriori* and never would have found it if you had not determined the orbit by other means."

Lastly, Bernoulli's anti-"metaphysical" tendency is still more strongly shown in a passage[94] of a letter to Euler of April 29, 1747:

"Herr Ramspeck has written to my father that you are engaged in various public metaphysical controversies. You really ought not to meddle with such matters, for from you we expect only sublime things, and it is not possible to excel in metaphysics."

Euler, we know, had a strong reverence for "metaphysics" and

würde. Der calculus aber wird ohne Zweifel weitläufig seyn, und bin ich von diesem principio nicht convincirt, so dass Ew. sich schwerlich die Mühe werden geben wollen meine Conjectur zu untersuchen. Wenn solche aber richtig wäre, würde es, wie ich glaube, leicht seyn, schier aller curvarum maxima et minima a priori anzuzeigen" (*ibid.*, pp. 533-534).

[93] "Ich zweifle ob man jemals a priori werde zeigen können, dass die elastica müsse maximum solidum generiren; ich betrachte solches als eine Proprietät, die der calculus ausweiset, und die kein Mensch ex principiis novis jemals würde haben können vorhersehen, eben so wenig als die identitatem isochronae et brachystochronae. Dergleichen proprietates sind ratione nostri gleichsam accidental, und auf diesen Fuss betrachte ich auch die observatam proprietatem orbitarum, in quibus $\int u.ds$ ein minimum macht, worin ich um so viel mehr confirmirt werde, als ich errathen, dass Sie diese proprietatem nur a posteriori observirt haben und niemals würden gefunden haben, wenn Sie nicht die orbitas aliunde determinirt hätten" (*ibid.*, p. 543).

[94] "Herr Ramspeck hat meinem Vater geschrieben, dass Sie in unterschiedenen controversiis metaphysicis publicis stehen. Sie sollten sich nicht über dergleichen Materien einlassen; denn von Ihnen erwartet man nichts als sublime Sachen, und es ist nicht möglich in jenen zu excelliren" (*ibid.*, p. 621).

consequently attached to Maupertuis's *a priori* speculations a value far above his own discovery. We shall see later that, in papers published among the *Mémoires* of the Berlin Academy, he emphasizes, as he apparently did to Daniel Bernoulli, the fact that he had only discovered the minimal condition satisfied by orbital motion in an *a posteriori* manner, as if this was rather a demerit. Nowadays we would say that Euler's great caution in, for example, insisting, in his *Methodus,* that the v in

$$\int v.ds$$

is to be expressed in terms of s by the principle of *vis viva,* so that his minimal principle cannot be extended to the case of motion in a resisting medium, where the principle of *vis viva* does not hold, and, in later publications, the careful enumeration of cases when testing Maupertuis's statical principle, are merits. But the following extract from the first appendix on elastic curves to the *Methodus* of 1744 proves that more general "metaphysical" ideas were by no means foreign to Euler:

"For since the plan of the universe is the most perfect possible and the work of the wisest possible creator, nothing happens which has not some maximal or minimal property, and therefore there is no doubt but that all the effects in nature can be equally well determined from final causes by the aid of the method of maxima and minima as from the efficient causes."[95]

X.

We will now return to the publications of the Berlin Academy.

The only paper concerning us here in the *Histoire* for 1747,

[95] "Cum enim Mundi universi fabrica sit perfectissima, atque a Creatore sapientissimo absoluta, nihil omnino in mundo contingit, in quo non maximi minimive ratio quaepiam eluceat; quamobrem dubium prorsus est nullum, quin omnes Mundi effectus ex causis finalibus, ope Methodi maximorum et minimorum, aeque feliciter determinari queant, atque ex ipsis causis efficientibus, *Methodus,* p. 245, and cf. section XII below. (See *Ostwalds Klassiker,* No. 175, p. 18. Cf. Mach, *Mechanik,* p. 485; *Mechanics,* p. 455. Cf. also E. Dühring, *Kritische Geschichte der allgemeinen Principien der Mechanik,* 3d ed., Leipsic, 1887, pp. 293-294, 296-299, 385-400). These reflections of Dühring's are on the effects of philosophy on mechanics and Lagrange's anti-"metaphysical" tendencies. Lagrange's own words are (*Méchanique analitique,* Paris, 1788, p. 187): ". . . . as if vague and arbitrary denominations [such as *the least quantity of action*] made up the essential part of the laws of nature and could by some secret virtue raise simple results of the known laws of mechanics to the position of final causes"; and (p. 189): ". . . . I regard this principle [of least action] not as a metaphysical principle but as a simple and general result of the laws of mechanics."

On the principle of least action with Fermat, Maupertuis, Euler, and Lagrange, and its effect on Gauss, cf. Dühring, *op. cit.,* pp. 100-102, 218-219, 287-302, 425-430.

published in 1749, is one in the class of speculative philosophy by Samuel Formey,[96] entitled: "Examen de la preuve qu'on tire des fins de la nature, pour établir l'existence de Dieu"; in which the author comes, by a rather different way, to the same conclusions as Maupertuis (1746).

In the *Histoire* for 1748, published in 1750, there are two papers relating to our subject by Euler.[97] The first is entitled: "Recherches sur les plus grands et plus petits qui se trouvent dans les actions des forces," and he quoted with approval Maupertuis's memoir of 1746, and remarked[98] that Maupertuis had shown that in the state of equilibrium of bodies, if some small movement were to happen to them, the quantity of action would be the least. He himself, says Euler, had discovered a similar law in the motion of bodies attracted to one or many centers of forces; in this case $\int u . ds$ expresses the quantity of action. In statics[99] this principle has been long recognized. Thus, it is easy to see that a chain suspended by its ends must take such a figure that the center of gravity of the chain is as low as possible; and thus, if x is the distance of the element ds from an arbitrary horizontal plane, $\int x . ds$ will be a minimum for the curve of the chain, and $\int z . ds$ is the quantity of action.[100] Many other analogous cases were, according to Euler, treated by Maupertuis; and Daniel Bernoulli remarked that the curve of an elastic lamina has a minimal property, and this view was developed by Euler in Appendix i of his *Methodus inveniendi* of 1744.[101]

There are, then, two ways of solving mechanical problems: one is the direct method, and the other is, knowing the formula which must be a maximum or a minimum, by the method of maxima and minima; the effect is determined by efficient causes and by final causes respectively. But it is often very difficult to discover the formula which must be a maximum or a minimum, and by which the quantity of action is represented; and this investigation belongs

[96] Pp. 365-384.

[97] Pp. 149-188 and 189-218.

[98] *Ibid.*, p. 150.

[99] *Ibid.*, pp. 150-151.

[100] *Ibid.*, p. 151.

[101] A convenient German translation of this Appendix, with critical and historical notes by H. Linsenbarth, was given in No. 175 of *Ostwalds Klassiker* (*Abhandlungen über das Gleichgewicht und die Schwingungen der ebenen elastischen Kurven von Jakob Bernoulli (1691, 1694, 1695) und Leonh. Euler (1744)*). Very interesting are Euler's (pp. 18-20) theological remarks and references to the frequency with which maximal and minimal problems appeared in the mechanical work of the Bernoullis. (Cf. section IX above.)

rather to metaphysics than to mathematics. "I believe," says Euler,[102] "that we are still very far from that degree of perfection where we are able to assign, for each effect which nature produces the quantity of action which is the smallest, and deduce it from the first principles of our knowledge; and that it will be almost impossible to arrive at it unless we discover, for a great number of different cases, the formulas which become maximal or minimal. Now, knowing the solutions with which the direct method furnishes us, it will not be difficult to find *a posteriori* formulas which express the quantity of action, and then it will not be so difficult to prove their truth by the known principles of metaphysics." With this end in view, Euler investigated several problems as to the curve formed by a flexible string in equilibrium.

Euler[103] arrived at the conclusion that the expression of the quantity of action, which, when supposed to be a minimum, gives the figure of the thread, is in perfect agreement with the *Law of Rest* published by Maupertuis in 1740.

Euler's second memoir on the principle of least action in this volume is entitled: "Réflexions sur quelques Loix générales de la Nature qui s'observent dans les Effets des Forces quelconques." He emphasizes[104] that he was only led *a posteriori* to the discovery of the minimum in the case of the equilibrium of threads, and then[105] remarks: "It is the figure which a fluid mass, all of whose particles are attracted by any forces, which was the principal object of the researches of M. de Maupertuis in order to discover the general law of rest in the Paris *Mémoires* of 1740. Thus I too will consider a fluid mass, all of whose particles are attracted to as many fixed centers as is wished by forces proportional to any functions of the distances to those centers, and I will investigate the figure of equilibrium for this mass. Then I will try to discover what will be a maximum or a minimum in this figure, in order to be in a better state to determine what must be understood by the name of the *quantity of action of the attracting forces*; and afterwards I will show by some reflections the great importance of this quantity in all researches concerning the effects produced by any forces." The expression discovered in this way was again found to agree with Maupertuis's law of 1740.

[102] *Op. cit.*, p. 152.

[103] *Ibid.*, p. 180.

[104] *Ibid.*, p. 190.

[105] *Ibid.*, p. 191; cf. p. 190.

XI.

There is nothing relating to the principle of least action, nor to mechanics (except in astronomy) in the Berlin *Histoire* for 1749 (published in 1751); but in that for 1750 (published in 1752) there is[106] an "Exposé concernant l'examen de la lettre de M. de Leibnitz, alleguée par M. le Prof. Koenig[107] dans les mois de Mars, 1751, des Actes de Leipzig,[108], à l'occasion du principe de la moindre action" by Euler,[109], with the note: "As will easily be seen by reading this memoir, it is one of those whose publication may not be delayed."

König had denied the validity of the principle in the case of equilibrium, and indicated some cases in which what, according to the principle, ought to be a minimum really reduces to nothing. But, says Euler,[110] "this objection is not of great importance, since it is sufficiently recognized in the calculus of maxima and minima that it can often happen what is a minimum vanishes entirely. But although that may be so in certain cases it by no means results that one ought to extend it to all cases of equilibrium, as always necessarily happening in that state; on the contrary, there are numberless cases in which this quantity of action is not zero but is really a minimum; and this puts beyond doubt that the aim of Nature is not the nullity of action, but its minimity." Then Euler quotes the example of the catenary, and says that the quantity of action reduces

[106] Pp. 52-64.

[107] Johann Samuel König (1712-1757); Cf. M. Cantor, *op. cit.*, pp. 599-601. König was a pupil of Johann Bernoulli's at the same time as Maupertuis. (Mayer, *op. cit.*, pp. 17-18).

[108] "De Universali Principio Aequilibrii et motus in Vi viva reperto deque nexu inter Vim vivam et Actionem utriusque Minimo" (*Nova Acta Eruditorum*, 1751, pp. 125-135, 144, 162-176). König affirms that equilibrium is a result of the *nullity* of action and *vis viva* (pp. 126, 164) that in some cases the action is a *maximum*, and this would hardly be reconcilable with Maupertuis's proof of the Creator's wisdom (pp. 126, 165); and that since *action* is *vis viva* into the time, the principle is that *vis viva* is a minimum (p. 127). König, like a thorough Leibnizian, praises the theorem of *vis viva* highly ("Censeo itaque, Theoremate Virium vivarum fundamentum universae Mechanicae contineri," p. 169), and deduces statistics from it. The extract from the letter of Leibniz's is given quite at the end (p. 176) and is: "L'Action n'est point ce que vous pensés, la considération du tems y entre; elle est comme le produit de la masse par le tems, ou du tems par la force vive. J'ai remarqué que dans les modifications des mouvemens elle devient ordinairement un Maximum, ou un Minimum. On en peut deduire plusieurs propositions de grande consequence; elle pourroit servir à determiner les courbes que decrivent les corps attirés à un ou plusieurs centres. Je voulois traiter de ces choses entr'autres dans le seconde partie de ma Dynamique, que j'ai supprimée; le mauvais accueil, que le prejugé a fait à la premiere, m'ayant degouté."

[109] As we learn from a note on p. 63 of the *Histoire* for 1750.

[110] *Ibid.*, p. 53.

to the distance of the center of gravity of the chain from the center of the earth; and[111] Daniel Bernoulli's and his own researches on elastic curves.

As regards dynamics, König quoted from a supposed letter written by Leibniz to Hermann, in which "action" was defined as Maupertuis defined it and the property of being "ordinarily a maximum or a minimum" in dynamical problems remarked. König could not produce the original nor could the original be found by officials. It is not interesting now to follow the controversy much further. König did not charge Maupertuis with plagiarism;[112] but, since the principle was considered by Maupertuis and others to be of the greatest possible importance and to reflect great credit on Maupertuis, its discoverer, the Berlin Academy, of which Maupertuis was president, took up the matter with great zeal, and concluded, like Euler's report, that, on internal and external evidences, the fragment of the letter was forged, either to injure Maupertuis or to exaggerate, by a pious fraud, the merits of Leibniz.[113] The result was an unjust expulsion of König from the Berlin Academy, and the consequent culmination of Voltaire's ill-feeling towards Maupertuis.[114]

XII.

To return to the *Histoire* for 1750. To the literature of the controversy also belongs a "Lettre de M. Euler à M. [Jean Bernard] Merian" of September 3, 1752.[115] Nowadays, the only interesting part of this letter is where Euler[116] gives some details about the publication of his *Methodus inveniendi.* The defenders of König stated that they knew the *Methodus* had been in the publisher's hands at Lau-

[111] *Ibid.*, p. 54.

[112] *Ibid.*, p. 60.

[113] *Ibid.*, p. 62.

[114] On the König incident, see La Beaumelle, *op. cit.*, pp. 139-141, 143-145, 150-167, and, on Voltaire's part in it, pp. 167 *et seq.* Further du Bois-Reymond, *op. cit.*, pp. 35-36, 47, 50-66. It is now known that the fragment of Leibniz's letter was probably genuine, and part of a letter to Varignon; Cf. *ibid.*, pp. 56-57, and the references to Gerhardt's paper in M. Cantor, *op. cit.*, p. 599. Even in 1877, Mayer (*op. cit.*, p. 19) said that the letter was without doubt forged; but Helmholtz in 1887 (*op. cit.*) showed that its genuineness was probable.

It appears that Euler only made one separately printed contribution to the discussion on König's dissertation; it is entitled: "Dissertatio de principio minimae actionis una cum examinatione objectionum Cl. Prof. König contra hoc principium factorum," Berlin, 1783. We have not seen this work, but only quote it from the Bibliography in Fuss's *Correspondance*, vol. i, p. xciv.

[115] *Ibid.*, pp. 520-532.

[116] *Ibid.*, pp. 525-526.

sanne since 1743, a circumstance which would give Euler priority over Maupertuis. This, says Euler, is correct in so far as it concerns the treatise itself, which he had finished some years before it appeared, but he only made the additions since he had sent the manuscript to Lausanne, and only shortly before the publication of the book towards the end of 1744. Further, he had communicated this supplement to nobody before printing it.

"When," says Euler,[117] "I used the method of maxima and minima to define the trajectories which are described by bodies attracted by any central force, I do not pretend to have been beyond what MM. Bernoulli and others have done when they determined by the help of the same method the curvature of the catenary, that of a piece of linen filled with liquid, and other curves of the same kind. Such investigations only furnish particular principles which can hardly be extended further than the cases to which they are applied. On the other hand, it is a question here of a universal principle, from which all the former principles should result, and which can be regarded as a Law established in all the phenomena of nature; which would render its discussion less the part (*du ressort*) of Mathematics than of Metaphysics, on the principles of which this doctrine should be founded. Also, although for long people have not doubted that, in all natural effects, there is a maximal-minimal principle which determines them, nobody before the Illustrious President of our Academy has even suspected in what elements this principle was contained and how it could be accommodated to all cases.[118] As

[117] *Ibid.*, pp. 526-527.

[118] Cf. *Methodus*, pp. 309, 320. The actual quotations are: (1) "Quoniam omnes naturae effectus sequuntur quandam maximi minimive legem; dubium est nullum, quin in lineis curvis, quas corpora projecta, si a viribus quibuscunque sollicitentur, describunt, quaepiam maximi minimive proprietas locum habeat. Quaenam autem sit ista proprietas, ex principiis metaphysicis a priori definire non tam facile videtur: cum autem has ipsa curvas, ope Methodi directae, determinare liceat; hinc, debita adhibita attentione, id ipsum, quod in istis curvis est maximum vel minimum, concludi poterit. Spectari autem potissimum debet effectus a viribus sollicitantibus oriundus; qui cum in motu corporis genito consistat, veritati consentaneum videtur hunc ipsum motum, seu potius aggregatum omnium motuum qui in corpore projecto insunt, minimum esse debere. Quae conclusio etsi non satis confirmata videatur, tamen, si eam cum veritate jam a priori nota consentire ostendero, tantum consequetur pondus, ut omnia dubia quae circa eam suboriri queant penitus evanescant. Quin-etiam cum ejus veritas fuerit evicta, facilius erit in intimas Naturae leges atque causes finales inquirere; hocque assertum firmissimis rationibus corroborare."...... (2) "Tam late ergo hoc principium patet, ut solus motus a resistentia medii perturbatus excipiendus videatur; cujus quidem exceptionis ratio facile perspicitur, propterea quod hoc casu corpus per varias vias ad eundum locum perveniens non eandem acquirit celeritatem. Quamobrem, sublata omni resistentia in motu corporum projectorum, perpetuo haec constans proprietas locum habebit, ut summa omnium motuum elementarium sit

regards myself, I only knew in a sure manner *a posteriori* the principle I used to determine trajectories; and I have ingenuously confessed that I was not in a position to establish its truth in another manner. All that I have done is to deduce from it the same curves that are commonly found by the direct method, starting from the principles of mechanics. I have not even dared to extend its use unless I could justify by calculation its agreement with known principles. And that is what has led me to separate from this principle motions in a resisting medium and other more complicated ones; for no way presented itself to my mind of discovering the truth with regard to these motions."

Among the *Mémoires* in the Class of Speculative Philosophy in the same volume (1750) of the *Histoire,* are two by Merian[119] entitled: "Dissertation ontologique sur l'Action, la Puissance et la Liberté," and "Seconde Dissertation sur l'Action, la Puissance et la Liberté"; in the first of which[120] Maupertuis's explanation, in the *Essai de Cosmologie,* of the generation of the idea of motive force is quoted.

XIII.

In the Berlin *Histoire* for 1751, published 1753, there are five memoirs we shall have to notice, and all of the Class of Mathematics.[121]

The first is by Euler,[122] and is entitled: "Harmonie entre les Principes généraux de Repos de Mouvement de M. de Maupertuis." Both principles of Maupertuis (of 1740 and 1744) rest, says Euler, on the same foundation, so that if one is proved, the other cannot be

minima. Neque vero haec proprietas in motu unius corporis tantum cernetur, sed etiam in motu plurium corporum conjunctim; quae quomodocunque in se invicem agant, tamen semper summa omnium motuum est minima. Quod, cum hujusmodi motus difficulter ad calculum revocentur, facilius ex primis principiis intelligitur, quam ex consensu calculi secundum utramque Methodum instituti. Quoniam enim corpora, ob inertiam, omni status mutationi reluctantur; viribus sollicitantibus tamparum obtemperabunt, quam fieri potest, siquidem sint libera; ex quo efficitur, ut, in motu genito, effectus a viribus ortus minor esse debeat, quam si ullo alio modo corpus vel corpora fuissent promota. Cujus ratiocinii vis, etiamsi nondum satis perspiciatur; tamen, quia cum veritate congruit, non dubito quin, ope principiorum sanioris Metaphysicae, ad majorem evidentiam evehi queat; quod negotium aliis, qui Metaphysicam prositentur, relinquo."

[119] Pp. 459-485 and 486-516.

[120] *Ibid.,* p. 479.

[121] In this volume, the memoirs in the Classes of Experimental Philosophy and Mathematics are paged (pp. 1-356) separately from those in the Classes of Speculative Philosophy and of Belles Lettres (pp. 1-154).

[122] Pp. 169-198.

doubted. Now, Maupertuis and Euler had established the truth of the law of rest of 1740 by a multitude of different cases. Euler, then, first deduced the principle of motion from that of rest,[123] and then[124] showed that all the elementary theorems of statics follow readily from the law of rest.

The nerve of Euler's investigation is the deduction of the principle of least action from the law of rest. Euler[125] called the integral $\int V.dv$, where V is a central force acting on the body M and v is the distance from M to any fixed point in the direction of V, the *effort* (effort), so that Maupertuis's law is that the sum of all the efforts is a maximum or a minimum.

"What is more natural," exclaims Euler,[126] "than to maintain that this same principle of equilibrium should also subsist in the movement of bodies under like forces? For if the intention of nature is to economize the sum of the efforts as much as possible, this intention must extend also to movements, provided that we take the efforts, not merely as they subsist in an instant, but in all the instants together for which the movement lasts. Thus, if the sum of the efforts is Φ for any instant of the motion, then, putting dt for the element of the time, the integral $\int \Phi.dt$ must be a minimum. If then, for the case of equilibrium the quantity Φ must be a minimum, the same laws of nature seem to exact that, for motion $\int \Phi.dt$ should be the smallest.

"Now it is precisely in this formula that the other principle of M. de Maupertuis, concerning motion, is contained, however different it may appear at the first glance. To show this agreement, I have only to remark that when a body moves under the action of the forces V, V′, V″,...., the effort Φ to which the body is subject expresses at the same time the *vis viva* of the body— the product of the mass M of the body and the square of its velocity (u)." Thus the formula which must be a minimum is

$$\int M.u^2.dt = \int M.u.ds.$$

Where v, v', v'',...., are the distances of M from the centers

[123] On pp. 181-182, Euler remarked that, if we wish, inversely, to deduce the principle of rest from that of motion, "we must suppose the motion infinitely small, and this causes great obscurities (*brouilleries*) in the consideration of infinitely small velocities and of the spaces which are described in an infinitely small time.

[124] *Ibid.*, pp. 183-193.

[125] *Ibid.*, p. 174.

[126] *Ibid.* p. 175.

of forces V, V′, V″,...., which are functions of these distances, Euler[127] gets the equation

$$Mu^2 = \text{const} - \Sigma \int .dv = \text{const} - \Phi;$$

and:[128] "the constant does not disturb this harmony between the effort Φ and the *vis viva* $M.u^2$ of the body; for if $\int \Phi.dt$ is a maximum or a minimum, $\int M.u^2.dt$ or $\int M.u.ds$ will be so also, since the term $\int \text{const}\, dt = \text{const}\ t$ does not enter into the consideration of the maximum or minimum. And, besides that, as the effort Φ is expressed by integral formulae, it already contains in itself any constant, so that I could have neglected this constant entirely and simply put $Mu^2 = -\Phi$, whence the identity would have been more evident. However, if we take the above integrals on a fixed footing (*sur un pied fixe*), so that the effort Φ receives a determined value, the addition of the constant will be necessary; since the velocity of the body at a certain point of its path depends on the initial velocity, and by this initial velocity the constant must be determined in each case proposed. But, of whatever quantity it may be, the determination of the maximum or minimum is not affected." Of course, as Mu^2 is equal to the negative of Φ, if $\int Mu^2.dt$ is a minimum, $\int \Phi.dt$ will be a maximum, and reciprocally.

Euler[129] then proved "the identity between the effort and the *vis viva*" for two or more bodies, connected in any way with one another to make a flexible body: the sum of the *vires vivae* of all the elements of the body always reduces to the sum of the efforts to which all the elements are subject in the same time,—in the case of two bodies of masses M and N, distances to the[130] center of force considered x and y respectively, and the accelerating forces X (a function of x) and Y (a function of y) respectively,

$$\Phi = M\int X.dx + N\int Y.dy.$$

Euler[131] remarked that there are cases of equilibrium in which the sum of the efforts is a *maximum* and[132] classes the cases of equilibrium as of such natures that, if the sum of efforts is a minimum, equilibrium reestablishes itself on an infinitely small dis-

[127] *Ibid.*, p. 177.

[128] *Ibid.*, p. 178.

[129] *Ibid.*, pp. 179-181.

[130] Of course the proof extends to as many centers of force as wished.

[131] *Ibid.*, p. 194.

[132] *Ibid.*, p. 195. There is an example of the sum of efforts being a maximum on pp. 195-196.

placement being given to the system, whereas, if the sum is a maximum, this is not the case.[133]

XIV.

Euler's second paper in the volume for 1751 is entitled: "Sur le Principe de la Moindre Action."[134] This paper is concerned with the opinion that there is a minimum in the actions of nature, with Aristotle and his school, Descartes, Fermat, Leibniz,[135] Wolff, Engelhard, s'Gravesande, and others, and was occasioned by the König affair. It is ridiculous, says Euler,[136] to suppose that König's fragment was written by Leibniz, for it attributes to Leibniz a principle opposed to that which he adopted publicly in the case of the motion of light—that the product of the path described and the resistance is a minimum.

Referring to his own discovery of the minimum of the action—integral for central orbits, Euler[137] remarks: "Besides, I had not discovered this beautiful property *a priori* but (using logical terms) *a posteriori*, deducing after many trials the formula which must become a minimum in these movements; and, not daring to give it more force than in the case which I had treated, I did not believe that I had discovered a wider principle: I was content with having found this beautiful property in the movements which take place around centers of forces."

Euler's third paper in this volume is entitled: "Examen de la Dissertation de M. Le Professeur Koenig, inserée dans les Actes de Leipzig, pour le Mois de Mars, 1751."[138] In this paper Euler examined König's demonstrations with care and pronounced them to be worthless.[139]

The "Essai d'une Démonstration Métaphysique du Principe général de l'Equilibre" of Euler, printed in the same volume,[140] does not mention Maupertuis's name,[141] and is concerned with the deduction from indubitable axioms of the principle that, for equilib-

[133] Cf. Mach, *Mechanik*, pp. 70-75; *Mechanics*, pp. 69-73.
[134] *Loc. cit.*, pp. 199-218.
[135] *Ibid.*, pp. 205-209.
[136] *Ibid.*, p. 209.
[137] *Ibid.*, p. 214.
[138] *Ibid.*, pp. 219-239, "Additions," pp. 240-245.
[139] *Ibid.*, p. 220.
[140] *Ibid.*, pp. 246-254.
[141] It is, however, Maupertuis's "Law of Rest" (Cf. also Mayer, *op. cit.*, p. 23).

rium, where, P, Q,.... are forces and x, y,.... are measured on their respective lines of action,

$$\int P.dx + \int Q.dy + \ldots.$$

is a minimum.

Lastly, there is, in this volume a paper by Nicolas de Beguelin,[142] tutor of Frederick the Great's nephew who was later Frederick William II, entitled: "Recherches sur l'Existence des Corps Durs,"[143] in which Maupertuis is called a great man[144] and the illustrious author of the principle of least action,[145] and the other conclusions are just what Maupertuis would have wished.

XV.

In the Paris *Mémoires* for 1749, the Chevalier d'Arcy[146] published some reflections on the principle of least action, which he had long hesitated to publish, but that he did so in the interests of truth. D'Arcy maintained: (1) That the action of a body is not proportional to $m.v.s$, because this supposition, in a particular case, leads to a result contrary to that which the laws of motion give; (2) That, admitting Maupertuis's definition of action, the quantity of it that nature employs in each change is not a minimum, and that if in some cases this is so, the principle of least action cannot serve to prove it; (3) that Maupertuis's law of equilibrium that Maupertuis deduced from the principle of least action is only established by the introduction of a foreign and gratuitous supposition; (4) that, in general, whatever may be the laws of nature, one could always easily find a function of the masses and velocities which would represent them when it is supposed to be a minimum, but this property would not be enough to give the name of *action* to this function nor to raise the principle thence obtained to the rank of a metaphysical principle;[147] (5) that, if we define the *action of*

[142] Lived from 1714 to 1789. (Cf. Berlin *Histoire*, 1788-9 (not seen); M. Cantor, *op. cit.*, vol. iv, 1908, pp. 174 (article by F. Cajori), 227 (article by E. Netto).

[143] *Ibid.*, pp. 331-355.

[144] *Ibid.*, pp. 344, 346.

[145] *Ibid.*, p. 347.

[146] "Réflexions sur le Principe de la moindre Action de M. de Maupertuis," *Hist. de l'Acad. Roy. des Sci.*, 1749 (Paris, 1753), *Mémoires*, pp. 531-538. There is an account of this memoir in the *Histoire*, pp. 179-181. Patrick d'Arcy was born on Sept. 18 (27), 1725, at Galloway and died on Oct. 18, 1779. He was a count, a field marshal of France, and a "Pensionnaire-Géomètre" of the Paris Academy (*Poggendorffs biog.-lit. Handwörterbuch*, vol. i, p. 57). Cf. M. Cantor, *op. cit.*, vol. iv, 1908, p. 18 (article by S. Günther).

[147] *Ibid.*, pp. 535-536.

a body around a point to be the product *m.v.p,* where *p* is the perpendicular drawn from this point on the direction of the *body,* then the *total action existing in nature at any instant around a given point, being produced in one given body, the quantity of action of this body will always be the same around this point,*[148] and from this theorem are easily deduced the principle of the conservation of *vis viva,* the case of rest, the centers of oscillation or of percussion, the law of the refraction of light, and so on.

With regard to (1), d'Arcy[149] gave the following considerations. "If two bodies produce equilibrium, that is to say, if rest follows from their direct impact, without our knowing to what the action is proportional, it (the action) must necessarily be equal in the two bodies; for if not, then it would follow that an action was in equilibrium with a lesser action, that is to say that different actions produce the same effect. Now, can we imagine that two equal and similar effects can be produced by unequal quantities of causes? This does not imply that the effect is proportional to its cause, but only that the same effect is always produced by the same quantity of cause and *vice versa.*

"Let there be two hard bodies A and B perfectly equal and proceeding in opposite directions with equal velocities, then clearly rest will follow their impact. If A, proceeding in the same direction with the same velocity, is impinged upon by the body C of different mass and velocity, but such that rest follows impact, I believe that nobody can deny that the action of B is equal to that of C, since both destroy the velocity of A. Can we have another idea of the equality of two quantities than of our being able to substitute one for the other without changing anything?" If B proceeds with double the velocity of, and traverses double the space traversed by, C, the principle of Maupertuis says that the mass of C is four times that of B; and this is contrary to what we find by the laws of motion. "Thus," concludes d'Arcy, "the action is not proportional to the mass multiplied by the velocity and by the space described."

With respect to (2), d'Arcy[150] remarked that if two bodies A and B proceed in the same direction with the velocities *a* and *b,*

[148] This theorem was given by d'Arcy in the Paris *Mémoires* for 1747 (published in 1752; pp. 348-356) under the title: "Principe général de Dynamique, qui donne la relation entre les espaces parcourus et les temps, quelque soit le systeme de corps que l'on considère, et quelles que soient leurs actions les uns sur les autres." This memoir (read in 1746) is part of the paper (of three memoirs) entitled: "Problème de Dynamique" on pp. 344-361.

[149] *Loc. cit.,* pp. 532-533.

[150] *Ibid.,* pp. 533-534.

the action of the bodies A and B will be Aa^2+Bb^2. If after impact they proceed with the velocities x and z, their action after impact will be Ax^2+Bz^2.[151] Now the quantity of action after impact will be either equal to or less than or greater than what it was before impact: if it is equal we have the theorem of *vis viva*, which does not hold for hard bodies; if it is greater it will have increased by the quantity

$$Ax^2+Bz^2-Bb^2-Aa^2;$$

if it is smaller it will be diminished by the quantity

$$Aa^2+Bb^2-Ax^2-Bz^2,$$

and this quantity is the real quantity of action lost, and consequently is that employed by nature to produce the actual change; therefore

$$2Ax.dx+2Bz.dz=0,$$

or, if we suppose $dx=dz$,[152]

$$Ax+Bz=0,$$

which is absurd. It is not, then, the destroyed part of this quantity which is a minimum. Maupertuis's argument is: Suppose that the bodies A and B proceed in the same direction with the velocities a and b and that the plane on which they are moves with the velocity x; evidently A will move on this plane with a velocity $a-x$ and B will move behind with a velocity $x-b$, x being greater than b and less than a. Maupertuis finds that

$$A(a-x)^2+B(x-b)^2$$

will be a minimum when the velocity x is such that

$$A(a-x)=B(x-b),$$

that is to say, when the bodies are in equilibrium on this plane. "I vow," said d'Arcy,[153] "that I do not know what consequence one can deduce from this other than: AP^2+BQ^2 being a minimum and $P^2=\int\Phi.dx$ and $Q^2=\int\Delta.dx$, we will have

$$A.\Phi+B.\Delta=0,$$

and consequently if

$$A.Z=B.X,$$

where Z and X are functions of x, then AZ^2+BX^2 will always be a minimum, and *vice versa*; and this leads me to believe that, when one has found that $A.Z^2+B.X^2$ is a minimum, one knew that $A.Z=B.X$."

[151] "Since a, b, x and z express the spaces as well as the velocities."

[152] For hard bodies $x=z$ and for elastic ones $a-b=z-x$.

[153] *Ibid.*, p. 534.

With regard to (3), when Maupertuis deduced the law of the lever from his principle of least action, he made a gratuitous supposition that the lever moves with a constant angular velocity.[154] To find the point of the lever (of length C) about which two bodies of masses A and B at the ends of the lever produce equilibrium, Maupertuis called Z the distance of A to the sought point, and announced that, to solve the problem, he would suppose the lever to receive some small movement and then express that the quantity of action is the smallest possible. If, remarked d'Arcy, we call V the small velocity of A and suppose that A describes a space α, the velocity of B and the space described by it will be, respectively,

$$V(C-Z)/Z \text{ and } \alpha(C-Z)/Z,$$

and the action of the bodies will be

$$AV\alpha + BV\alpha(C-Z)^2/Z,$$

and the differential equated to zero, supposing that α and V are constant, gives $Z=C$. Maupertuis gets the correct law by supposing that the lever moves with a constant angular velocity. But this supposition, says d'Arcy, "seems to me absolutely gratuitous, since, to each value of Z, the action or the time necessary for it to describe the constant angle is different."

With regard to (5), d'Arcy[155] remarks that his definition of action is in perfect agreement with d'Alembert's:[156] "The action is the movement that a body produces or tends to produce in another body."

D'Arcy's principle is that the sum of the masses of a system, each mass being multiplied by the sector which it describes around a fixed point in the same time, less the sum of the sectors described in the contrary sense, each being multiplied by the mass of the body which describes it, is proportional to the time. The only difference from the principle that d'Arcy gave in this memoir of 1749 is that instead of (as in 1747) sectors multiplied by masses, were used in 1749 the equivalent expressions $m.v.p$.

Let two bodies A and B move with the velocities a and b before impact and with the velocities x and z after impact. By the above principle the action of A and B round any point O will be the same after as before the impact; thus, where P is the foot of the perpendicular from O on the line joining A and B,

[154] *Ibid.*, p. 535.
[155] *Ibid.*, p. 536.
[156] In the *Encyclopédie* (not seen).

$$A.a.OP+B.b.OP=A.x.OP+B.z.OP,$$

and consequently

$$A(a-x)=B(z-b),$$

and this relation between the velocity lost by A and that gained by B holds whether the bodies are elastic or not. In elastic bodies we easily see that $a-b=z-x$, and hence, from the above equation

$$A(a^2-x^2)=B(z^2-b^2),$$

which is the property of *vires vivae.*[157]

If two bodies A and B strike the ends P and Q of a straight lever with the same velocity a, to find the fulcrum-point C of the bar such that A and B remain at rest after the impact, d'Arcy[158] observes that the action of A round C must be equal to the action of B round C, and thus C is the Center of gravity. By the same method we find the centers of oscillation or of percussion, and so on.

When deducing the law of the refraction of light,[159] d'Arcy observes that, in his memoir of 1747, he had proved that it is the same thing whether the bodies are attracted toward the point round which the action is sought or not, as the quantity of this action is not altered thereby. Let FG be the surface of a diaphanous and homogeneous sphere of center C, M a point outside the sphere, and N a point inside. A ray of light—μ being the mass of a corpuscle of light—travels from M to N, its velocity outside the sphere being v and inside the sphere being u, meeting the surface at m. "The action of the surface FG can only be towards the center C; for whatever action this body may have on the corpuscle of light on one side of the perpendicular to the surface, it will have the same action on the other side." Thus we have

$$\mu.v.CR=\mu.u.Cr,$$

and this gives the known law of refraction of light. The case of FG being plane instead of spherical is then treated, and d'Arcy finally remarks that other examples of the application of his principles are given in the memoir of 1747.

XVI.

The Berlin *Histoire* for 1752, published in 1754, contains among the memoirs of the class of Speculative Philosophy a "Réponse à un Mémoire de M. d'Arcy inséré dans le Volume de l'Académie Royale

[157] D'Arcy, *loc. cit.*, p. 537.
[158] *Ibid.*
[159] *Ibid.*, pp. 537-538.

des Sciences de Paris pour l'année 1749" by Maupertuis,[160] which is headed by a notice,[161] in italics, stating that the memoir (1744) in which the principle of the least quantity of action was first communicated was received by the Paris Academy, Maupertuis "dares to say, with some applause (*applaudissment*)." Then Maupertuis refers to his paper of 1746, to his *Essai de Cosmologie*, to the attacks of "un Professeur de la Haye" to whom, as he used libels, he will never reply, and to d'Arcy who "attacks with so much politeness and modesty," that Maupertuis thinks that he ought to reply to him. He appears, says Maupertuis, "to be such a lover of the truth that I will try to introduce him to it."[162]

(1) D'Arcy tried to show that Maupertuis is wrong to call $m.v.s$ *action*. Maupertuis believed that he had good grounds for justifying the name; but, to cut matters short, Maupertuis said that he had adopted Leibniz's definition.[163] D'Arcy's reason against calling the above product *action* reduces to this: In the impact of hard bodies, two different quantities of *action* reduce to rest one and the same body moving with the same velocity. By the same kind of reasoning, says Maupertuis, d'Arcy might object to the name *vis viva*; for two different *vires vivae* can reduce the same hard body to rest." And in fact here the *vis viva* is the same as the *action*, for here "the space is proportional to the velocity."[164] Again, with elastic bodies, if two unequal bodies with the same *vires mortuae* ($m.v$) strike a third body at rest, different *vires mortuae* will come into existence or perish.

(2) D'Arcy, to show that Maupertuis is wrong in holding that the quantity of action necessary to produce any change in nature is a minimum, confuses, when treating of impact, change of the quantity of action with change of velocities.[165] The velocities can change without the quantity of action changing, as is the case in the impact of elastic bodies (when this quantity is the same as the quan-

[160] *Histoire de l'Acad.* de Berlin, 1752, T. VIII, pp. 293-298.

[161] *Ibid.*, pp. 293-294.

[162] "....et paroit si Amateur de la vérité, que je tâcherai de la lui faire connoître" (*ibid.*, p. 294).

[163] "....mais pour trancher court avec M. d'Arcy, je puis dire que ce n'est pas mon affaire. Leibnitz, et ceux qui l'ont suivi, ont appelé ainsi le produit du corps par l'espace et par la vitesse; j'ai adopté une définition établie, contre laquelle on n'avoit point disputé, et que je n'avois aucune raison de changer; voilà ce qu'il me suffiroit de répondre"; *ibid.*, p. 295. Apparently this is upon what E. du Bois-Reymond relies when he says (*op. cit.*, p. 48): "Maupertuis borrowed, as he himself says, the concept and name of *action* from Leibniz..."

[164] *Ibid.*, p. 295.

[165] *Ibid.*, p. 296.

tity of *vis viva*); in the impact of hard bodies, the change of the velocities is neither equal nor proportional to the change in the quantity of action.

If[166] the bodies are elastic, the change is: A which moved before with the velocity a moves afterwards with the velocity α, and the corresponding velocities of B are b and β. If then we wish that afterwards A should move with the velocity a and B with the velocity b, we must transport the A-plane with the velocity $a-\alpha$ and the B-plane with the velocity $\beta - b$; and from this we must get the quantity of action $A(a-\alpha)^2+B(\beta-b)^2$ necessary to produce the change in nature, and which is a minimum. If A and B are hard, and the common velocity after the impact is x, and if we wish each body to move with its original velocity, we proceed as before, and get, for the quantity of action necessary to produce this change, $A(a-x)^2+B(x-b)^2$, the smallest possible.

(3) D'Arcy's criticism on Maupertuis's deduction of the lever is mistaken, for Maupertuis supposed the lever to be in a state of rest and infinitely little displaced from this state.[167]

Finally, Maupertuis[168] mentioned the incompleteness of this theory of the lever, which was not remarked by d'Arcy, but about which we have read in connection with the reprint of the memoir of 1740[169] in Maupertuis's *Œuvres.*[170]

XVII.

In the Paris *Mémoires* for 1752 appeared a reply by d'Arcy[171] to Maupertuis's paper in the Berlin *Mémoires* for 1752. After a few preliminary words in which what looks like sarcasm is veiled in words of compliment—Maupertuis's "modesty," "politeness," and "simplicity" being praised, d'Arcy[172] confesses that if he had need of a proof of an arranging intelligence he would find it just as much in the uniformity of the laws of generation of the vilest insects as in the general laws of mechanics.

[166] *Ibid.*, pp. 296-297.

[167] *Ibid.*, pp. 297-298.

[168] *Ibid.*, p. 298.

[169] Mapertuis here refers to this paper as being in the *Mémoires* for 1743. This is, of course, a misprint.

[170] See section II above.

[171] "Replique à un Mémoire de M. de Maupertuis, sur le principe de la moindre action, inséré dans les Mémoires de l'Académie royale des Sciences de Berlin, de l'année 1752," *Hist. de l'Acad. Roy. des Sci.*, 1752 (Paris, 1756), *Mémoires*, pp. 503-519.

[172] *Ibid.*, p. 503.

With regard to Maupertuis's (correct) classification of d'Arcy's objections under three heads, d'Arcy[173] maintains that the first still holds, for "when someone says that nature economizes action, he clearly means that this quantity expresses this cause or the real force," and d'Arcy[174] even accuses Maupertuis of falling back on the authority of Leibniz. His argument depends for its validity on the principle that a definition is something more than the mere giving of a name.

With regard to d'Arcy's second objection, d'Arcy[175] quoted from the *Encyclopédie*[176] to show that Maupertuis's phrase "change happened in nature" and that his own interpretation of this phrase in the above simple case of impact as

$$Aa^2+Bb^2-A\alpha^2-B\beta^2,$$

which is to be a minimum, so that

$$A\alpha+B\beta=0,$$

is natural and also showed[177] that Maupertuis himself implied this interpretation.

Then d'Arcy[178] showed that the manner in which Maupertuis used his principle in the case of the refraction of light is different from that in which he used it in the case of impact. If we treated the latter case like the former, we should have the result that

$$Aa^2+Bb^2+A\alpha^2+B\beta^2$$

is a minimum, and hence that

$$A\alpha^2+B\beta^2=0.$$

In the case of light, it is the action before the change plus the action after the change which is a minimum; in impact it is the mass by the velocity lost and by the space which will be described in consequence of this velocity.

With respect to Maupertuis's reply to d'Arcy's third objection, Maupertuis, says d'Arcy,[179] has misread the objection: there was not said to be a supposition about an *angular and constant* motion but about a *constant angular* motion. D'Arcy quotes objections nearly the same as his of 1749 from the above cited article on "Cosmo-

[173] *Ibid.*, p. 504.
[174] *Ibid.*, p. 506.
[175] *Ibid.*, pp. 507-508.
[176] Article "Cosmologie," p. 196 [not seen].
[177] D'Arcy, *loc. cit.*, pp. 508-509.
[178] *Loc. cit.*, pp. 509-510.
[179] *Ibid.*, pp. 510-511.

logie": "When Maupertuis applies his principle to the case of equilibrium in the lever, certain suppositions must be made, amongst others, that the velocity is proportional to the distance from the fulcrum,[180] and that the time is constant as in the case of impact...."

In the case of the reflection of light, d'Arcy[181] shows that nature is prodigal or avaricious of action as a mirror is more or less concave respectively, and again quoted the article "Cosmologie" on this point.

Finally, d'Arcy[182] returned to his principle of 1747, which he prepared to substitute for Maupertuis's principle.[183]

XVIII.

In the Berlin *Histoire* for 1753, published in 1755, the only paper[184] relating to the principle of least action is an "Examen des Reflexions de M. le Chevalier d'Arcy sur le Principe de la moindre action" by Louis Bertrand.[185] Bertrand's paper was headed by a note to the effect that, as the Paris Academy of Sciences had, contrary to its custom, hurried to publish in its *Mémoires* of 1749 some reflexions of d'Arcy which he had only given in 1752, the Berlin Academy believed that it might publish this examination one year before it ought to have appeared.

D'Arcy, says Bertrand,[186] undertook to overthrow Maupertuis's principle, but only succeeded in overthrowing the false ideas which he had formed about it. In the first place, d'Arcy objected to Mau-

[180] As d'Arcy expressed it, that the angular velocity is constant.

[181] *Ibid.*, pp. 511-513.

[182] *Ibid.*, pp. 513-519. On p. 513 he emphasized that the memoir containing this principle was read to the French Academy in 1746.

[183] On d'Arcy's memoirs see Mayer, *op. cit.*, pp. 13-15, 21. It seems to me that Mayer's view of these memoirs is too favorable. I will return to this point in my criticisms.

[184] The contrary was stated, owing to a wrong reading of A. Mayer, *op. cit.*, p. 17, by myself in *Ostwalds Klassiker*, No. 167, p. 36; but, of Euler's five papers in this volume, one is on Daniel Bernoulli's papers on vibrating cords (cf. M. Cantor, *op. cit.*, vol. iii, 2d ed., Leipsic, 1901, pp. 904-907), two papers are on spherical and spheroidical trigonometry deduced from the method of maxima and minima (cf. *ibid.*, pp. 867-869), one on the law of refraction of rays of different colors, and one on the paths of projectiles in resisting media;—and in none of these is any reference to the principle of least action except in a passage (p. 306) in the last line but one of these papers, where he refers to the convincing proof of the existence of a Deity given by Maupertuis, and also to the argument from the wonderful structure of the eye.

[185] Pp. 310-320. Louis Bertrand (1731-1812) was then in Berlin and was a friend of Euler's; cf. *Poggendorff*, vol. i, p. 171; M. Cantor, *op. cit.*, vol. iv, Leipsic, 1908, p. 332 (article by V. Bobynin).

[186] *Op. cit.*, p. 311.

pertuis's definition of action. This is a question of words;[187] d'Arcy required that the *action* of different hard bodies should be estimated equal if each of these bodies is capable of reducing to rest the same hard body endowed with a certain velocity, so that the *action* of a body is measured by $m.v$. But, says Bertrand, it is well known[188] that, in the impact of hard bodies, a part of the motion is destroyed —that part which would be reproduced if the bodies were elastic: "hence it follows that, if a hard body (A) of mass 1 and velocity 1 were reduced to rest both by a body (B) of mass 1 and velocity 1 and by a body (C) of mass ½ and velocity 2, we could only affirm positively that the action of B is equal to that of C if we have previously proved that when B impinges on A it loses the same quantity of motion as when C impinges on A. For if it were true that in one case more motion were lost than in the other, the rest in this case ought not to be attributed to the equality of action of the two bodies, but to the greater loss of motion; in fact, if this loss had not been greater, some motion would have been left for the bodies which have impinged, and thus rest would not have followed the impact.

"In order, then, that the reasoning by which M. d'Arcy has wished to prop up his definition of *action* should be conclusive, it would be necessary for him to prove that the same quantity of motion is lost whether B impinges on A or C impinges on A. Now this he will never prove.

"Not being able to do anything in that direction, perhaps he will claim that it is sufficient to attend to the change which happens to the body A after the impact; but, if he only pays regard to the effect produced on the body impinged upon, we can urge against him the impact of elastic bodies, where a body A of mass and velocity both 1 is reduced to rest both by a body B of mass 1 and velocity 0, by a body C whose velocity and mass are both ½, and by a body D whose mass is ⅓ and velocity 1. Now, M. d'Arcy would contradict his own definition of action if he claimed that the actions of B, C, and D were all equal to one another. Thus the foundation on which M. d'Arcy wished to support his manner of estimating action absolutely lacks solidity." In d'Arcy's last paragraph on the definition of action, he wrongly concludes, says Bertrand,[189], that from Maupertuis's definition of *action*, follows that whenever the

[187] *Ibid.*, pp. 311-312, 313.

[188] "....C'est une chose dont tous les Philosophes conviennent...." (*ibid.*, p. 312).

[189] *Ibid.*, pp. 313-314.

velocities and the masses of two hard bodies are such that rest follows the impact of these bodies, the actions of these bodies are equal.

With regard to d'Arcy's attack on Maupertuis's principle, Bertrand[190] remarks that Maupertuis expressly said that not the difference of the actions before and after the impact, but the quantity of action necessary to produce this change is to be a minimum. The quantity of action necessary to produce a change is not the difference of the actions before and after the change; but it is the product of the mass of the bodies whose state is changed, the space that these bodies describe in consequence of (*en suite du*) the change, and the velocity with which they describe it, also in consequence of the change.[191]

With regard to d'Arcy's strictures on Maupertuis's treatment of the lever, Bertrand[192] reproduces d'Arcy's supposition that A moves with a small velocity V and describes a space α, whence the velocity of B is $V(c-z)/z$, the space described by B is $\alpha(c-z)/z$, and the action of the whole system is

$$AV\alpha + BV\alpha(c-z)^2/z^2.$$

Then, before differentiating, d'Arcy supposed V and α constant; and Bertrand inquires why should the velocity of and space described by A be supposed to be constant rather than those of and by B. Maupertuis puts as constant the angle that A and B describe around the fulcrum of the lever; and this supposition does not affect one of the bodies rather than the other, for this angle is the same for both bodies. Still, this supposition appears gratuitous to d'Arcy because for each value of z the action or the time necessary to make A and B describe the angle supposed constant is different. But, says Bertrand, if the action necessary to make A and B describe the angle supposed constant were not different for each value of z, it would be absurd to seek which of these actions is the least.

With regard to d'Arcy's assertion that, whatever the laws of nature might be, it would always be easy to find a function of the velocities and masses such that, when minimized, it would give these laws, Bertrand[193] remarks that "that may be true of many particular cases." Rather earlier in his paper, Bertrand[194] remarks *à propos*

[190] *Ibid.*, p. 314.

[191] *Ibid.*, pp. 314-315.

[192] *Ibid.*, pp. 317-318.

[193] *Ibid.* p. 318.

[194] *Ibid.*, pp. 315-316.

of d'Arcy's suggestion that Maupertuis knew the formula $A(a-x)=B(x-b)$ for impact and concluded that the action must be

$$A(a-x)^2+B(x-b)^2$$

in order that the known formula should result when the action was minimized, and d'Arcy's attempted generalization, that, if Z and X are functions of x, then, if $AZ=BX$,

$$AZ^2+BX^2$$

will always be a minimum and *vice versa*, that this generalization will always be false except when $dZ+dX=0$,—the case which he wished to generalize.

The rest of Bertrand's[195] paper is devoted to d'Arcy's own principle. "This principle," says Bertrand,[196] "can in a certain sense be admitted, but it will never lead to important discoveries; still less will it show us, so to speak, the true ends in view of nature: and these circumstances put it infinitely below that of M. de Maupertuis."

With regard to the way in which Bertrand's paper is written, it seems both magisterial and hasty: attempts at sarcasm against d'Arcy and flattery—or perhaps sincere reverence—for Maupertuis stand out too prominently. Bertrand was young when he wrote it, so there is a greater chance that he was sincere. Still, he was of, or was about to be of, the Berlin Academy.

XIX.

We will now give a brief retrospect of the development of views on the principle of least action, and dispose of all historical questions before trying to elicit what gains have resulted for knowledge by this development.

A. Mayer[197] says of Euler's formulations of the principle of least action: "We shall see that this correct form [in the second appendix to the *Methodus* of 1744] got lost to him in the course of time, and that soon it lost as much in rigor as it appeared to gain in generality." Mayer's[198] grounds for this view were that Jacobi's[199] principle of least action was the "true" principle, owing to

[195] *Ibid.*, pp. 318-320. Just at the end is: "On pourroit faire encore nombre de réfléxions sur l'insuffisance de ce Principe appliqué à la réfraction des rayons de lumiére; mais il semble qu'il y auroit une sorte de mauvaise humeur à examiner si rigoureusement se que M. d'Arcy paroit avoir voulu traiter cavaliérement." I have left the accents unaltered.

[196] *Ibid.*, p. 319.

[197] *Op. cit.*, p. 6.

[198] *Op. cit.*, pp. 6-11.

[199] Cf. *Monist*, vol. xxii, April, 1912.

the difficulty there appeared to be[200] if the time was not eliminated, and this elimination had apparently to be done by the equation expressing the conservation of *vis viva.* Thus the principle of least action is subject to the limitations implied by the subsistence of the theorem of *vis viva.* Thus Euler, in the above mentioned appendix, expressly pointed out that his theorem cannot hold for motion in a resisting medium, and that, in the integrand, the velocity must be expressed "ex viribus sollicitantibus per quantitates ad curvam pertinentes.[201] Consequently Mayer[202] maintained that Lagrange's (1760) generalization of the principle of least action is, in the form in which Lagrange states it, meaningless, and the theorem which he really had in his mind is that known as "Hamilton's principle" given by Hamilton in 1835. We know[203] that later on (in a publication of 1886) Mayer changed this view, owing to acquaintance with a paper of Rodrigues's (1816) in which the time (the *t* in the integrand) was varied by the δ-process of the calculus of variations, and admitted that there are two forms of the principle of least action: Jacobi's and Lagrange's. This view has been confirmed by the later researches of Hölder.[204]

Now Jacobi's principle may be considered to be a generalized form of Euler's theorem, and Lagrange's principle a more precise and generalized form of Maupertuis's. So it happens that Maupertuis was right in thinking his theorem quite general, and Euler

[200] *Ibid.*

[201] *Methodus,* p. 312. Cf. pp. 318-319 on the necessity for the principle of *vis viva.*

[202] *Op. cit.,* pp. 26-29. Mayer (*ibid.,* p. 24) also remarked that Euler's later (Maupertuisian) form of the principle, in which the condition that all the quantities in the integrand must be reduced, by means of the principle of *vis viva,* to space-elements alone is not stated, is quite meaningless, for the forces acting on the system, on which the path of the system depends, do not occur in the integral of action. Here we will anticipate our criticism by pointing out that in Lagrange's memoir the conditoin

$$\delta T = \delta U,$$

where "T" and "δU" have the meaning already explained in *The Monist,* vol. xxii, April, 1912, p. 290, is *explicitly* given, and what would now be written in the same way was, tacitly or not, presupposed in all Euler's works. Mayer said that the problem of variations only subsisted under the condition

$$T = U + \text{const.},$$

which implies the preceding equation, but, as Lagrange pointed out, is not necessarily implied by it. And it is the preceding equation alone that we require to rescue the principle of least action from meaninglessness. Mayer's remark (*ibid.,* p. 27) that Lagrange completely leaves out the condition is simply an error.

[203] Cf. *Monist,* vol. xxii, April, 1912.

[204] *Ibid.*

was right in doing what Mayer[205] complains of—in dropping the condition about the theorem of *vis viva* holding.[206] Of course, it may have been, and probably was, the case that neither Maupertuis nor Euler had any good grounds for believing that they were right. Indeed, one is forced, against one's will, to the opinion that Euler was in a position in which, as Mayer[207] expresses it, "he could not with propriety retort to the powerful President of his Academy."

The only reason why it is necessary to inquire closely whether Euler really considered Maupertuis's principle to be valid seems to me mainly to be the provision of an example to show the necessity of an additional condition when we wish to deduce properties of motion from the equation of the variation of the integral of action to zero. There is also the possibility of our being given yet another example of the greater power of instinctive beliefs or "metaphysics" over the good man's mind than the love of scientific truth.[208] If we should have to conclude that Euler deliberately hid the truth for the personal favor of Maupertuis, this conclusion will fill us with the same regret and loathing that we feel for the weakness in Galileo's character and the disgraceful exercise of the church's power on him, respectively.

It seems to me true that Euler's love for "metaphysics" alone could not lead him to forsake scrupulous honesty in his search for the truth. It is difficult, but very possible, to acquit Euler of the charge of veiled sarcasm against Maupertuis's principle. In a paper, from which we have quoted above, in the Berlin *Mémoires* for 1748, he expresses his belief that we are still very far from being able to assign, for each effect which nature produces, the quantity of action which is the smallest, and from being able to deduce it from the first principles of our knowledge. Indeed, Euler seems to have no doubt that *something* must be a minimum, but he also thinks that this something may be different— or at least seem to us, without imperfect knowledge, different—in different cases.[209] At any rate Euler goes carefully through single statical cases and determines the equivalent in terms of the forces of "the quantity of

[205] *Op. cit.*, pp. 23-24. Euler did not, however, *explicitly* drop this condition.

[206] Euler had presupposed in 1744 that the principle of *vis viva* held: Maupertuis considered his principle applies to cases—such as the impact of inelastic bodies—where the principle of *vis viva* does not hold.

[207] *Ibid.*, p. 17.

[208] On Euler's "metaphysical" tendencies, cf. Mayer, *ibid.*, pp. 21-23.

[209] Cf. the remark of d'Arcy that, whatever the laws of nature might be one could always find a function of the masses and velocities which, when minimized, would represent them (cf. section XV).

action" in each case. Nowadays, we would say,[210] of course, that this inductive procedure was far more "reasonable" or scientific than Maupertuis's; but we must remember that then the opinion was far more generally held than it is now that knowledge of the truth could be attained by other than scientific methods.

It was, I think we must say, not merely love for "metaphysics" which led Euler to sacrifice important details of his principle. Comparison of Daniel Bernoulli's letter to Euler of September 4, 1743, with Euler's markedly different account in the Berlin *Mémoires* of 1750 of the circumstances about the publication of the *Methodus* of 1744, as well as Euler's obviously unjust attitude towards König, points to a lower influence. If we dismissed—as we would like—thoughts that this sort of influence came in, we would be faced with the insoluble problem that Euler supported a principle which was claimed to embrace cases where the theorem of *vis viva* fails while he had convinced himself that the subsistence of this theorem was a necessary condition for the validity of the principle. And here the suggestion arises of itself that, since Euler, in his papers in the Berlin *Mémoires*, only committed himself to the mathematical support—as distinguished from warmly expressed admiration—of Maupertuis's principle in *statical* cases, he dared not affirm that the action-integral was a minimum in nature even when the principle of *vis viva* did not hold.[211] This stop was reserved for Lagrange, and perhaps it was on this account that Euler in a letter of November 9, 1762, congratulated Lagrange in the words:[212] "What satisfaction would M. de Maupertuis not have, if he were still alive, to see his Principle of least action carried to the highest degree of dignity of which it is susceptible.[213] If this conjecture be true, we must believe that Euler had a childlike faith that "metaphysics" could generalize a theorem so far as to drop a condition which he had satisfied himself, was necessary. We know now that this faith —if indeed it existed—was justified.

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[210] Like Mayer, *op. cit.*, p. 23.

[211] Indeed, where he refers to dynamical cases (in the Berlin *Histoire* of 1751) he explicitly uses the principle of *vis viva*. Euler nowhere refers to the problem of the impact of inelastic bodies, on which Maupertuis and others laid such stress.

[212] *Œuvres de Lagrange*, vol. xiv, p. 201.

[213] "Quelle satisfaction n'aurait pas M. de Maupertuis, s'il était encore en vie, de voir son principe de la moindre action porté au plus haut degré de dignité dont il est susceptible."